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# Foragers' Pursuit of Individual Autonomy<sup>1</sup>

by Peter M. Gardner

Examination of 12 theories about the causes of foragers' individualism, egalitarianism, and social structural simplicity and flexibility reveals complementation of many of their main arguments and the possibility that diverse aspects of the pursuit of individual autonomy are interrelated. Two theories are, therefore, proposed and examined: (1) that some foraging cultures possess an individual-autonomy syndrome and (2) that foragers' pursuit of individual autonomy is multidetermined. Hologeistic cross-cultural comparison provides initial corroboration of the first theory, and preliminary work shows that the second has promise.

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1. A preliminary version of part of this paper was presented at the 86th annual meeting of the American Anthropological Association in Chicago, Ill., November 1987. I thank participants in that session, R. A. Benfer, and members of my 1990 Seminar in Ecological Adaptation for useful critical responses to early drafts; I take responsibility, of course, for problems which remain.

Many theories have been offered to account for foragers' apparent emphasis on individual autonomy—both the tendency toward it which is said to characterize Holocene hunters, fishers, and gatherers in general and the extreme emphasis on it which is reported for certain foragers in Asia, Africa, and North America. Despite decades of interest in the subject, no overall theoretical perspective has been achieved. Time has brought little clarification of either the relative power of the theories or the possible relationships among them. Indeed, some theories have been totally disregarded in the past few years, when those surveying the research in question (e.g., Smith and Winterhalder 1981:3-4; Morris 1982) have summed it up in terms of just two or three approaches. Following a review of diverse theories (four relatively general and eight more specific), three aspects of the relationships among the more complete of them will be taken up: (1) the significant complementation of many of their main arguments; (2) what might be termed an individual-autonomy syndrome; and (3) the idea that several processes may work conjointly, compounding each other's effects, thus multidetermining foragers' pursuit of individual autonomy.

## Theories, General and Specific

Four relatively general theories of foragers' pursuit of individual autonomy all bear upon subsistence economy:

1. *The adaptive-child-training theory.* Students of cross-cultural comparison have found that, while non-foragers tend to push children toward obedience and responsibility, foragers tend to press for self-reliance, independence, and individual achievement (Barry, Child, and Bacon 1959:52, 55-63; see also Whiting 1968:336-39; Rohner 1975:115-18). This difference suggests that child training is adapted to subsistence economy. Responsible behavior is advantageous to those who rely upon accumulated food resources; by contrast, independent initiative in procuring food is advantageous to foragers because their food quest begins each day anew. Except for Whiting's passing comment on the atypicality of the social structure of Australian foragers, this has been consistently presented as a theory about foragers in general.

2. *The nomadic-food-quest theory.* Lee and DeVore (1968:11-12) view foragers' egalitarian, flexible, individualized social life as being shaped by their nomadic food quest, dispersed and variable food resources, avoidance of food storage, and visiting between resource areas. Nomadic procurement of food, for instance, restricts the amount of personal property possible and minimizes interpersonal differences. With "the environment itself . . . [being] the storehouse" (p. 12), the distribution and supply of food lead to small flexible groups, with visiting and continual redistribution of population among the bands. Individual freedom of movement is allowed, and group fission is a ready way of resolving conflict. Elements of the theory go back at least to Forde (1947), and

Lee (1972) develops some of the ideas further, particularly as regards the ecology of spatial organizational flexibility.

3. *The foraging-mode-of-production theory.* Leacock and Lee (1982:7–9) cover a similar set of variables but phrase them in terms of mode of production. They argue that because of collective ownership of means of production, right of reciprocal access to the resources of others, limited accumulation, generalized reciprocity within and between camps, access to the forces of production, and a tempering of individual tool ownership by gift giving and exchange, band-living foragers around the world<sup>2</sup> exhibit certain social and ideological similarities. Their shared features include egalitarianism, “strong anti-authoritarianism,” “great respect for individuality,” and “marked flexibility in band membership and in living arrangements generally” (1982:7–8). Specific links between mode of production and superstructure are not, however, made clear.

4. *The resource-depletion theory.* Foley (1988), agreeing with those who interpret Late Pleistocene hunter-gatherers as having “greater social complexity than many recent hunter-gatherers” (p. 219), contends that resource depletion, an “increase in the importance of plant foods,” and perhaps a drop in food quality early in the post-Pleistocene underlie some interrelated changes in human adaptive strategy. What eventuated was “the small flexible band system, with general egalitarianism and both males and females contributing to the diet in approximately equal proportions” (p. 220).

Many have attributed an emphasis on individual autonomy (involving extreme individualism, egalitarianism, social structural simplicity and flexibility, and various combinations of these) to particular foraging peoples. In most cases the arguments are based on distributions: foragers said to exhibit such characteristics are the ones coping with certain kinds of food quest or culture contact. Eight relatively specific theories deserve review:

5. *The storage theory.* Cashdan (1980), offering a narrower version of the nomadic-food-quest theory, suggests that storage buffers against environmental variability. Comparing the nonegalitarian //Gana with other San, she argues that the egalitarianism regarded as typical of San results from “stringent constraints” arising from “high spatial and temporal variability in food supply, together with a paucity of means to buffer [i.e., to lessen the impact of] this variability” (pp. 116–17)—for instance, storage at home-base sites and individual accumulation of possessions. When the constraints are lifted (that is, when leveling mechanisms such as sharing, dunning for gifts, and scorning of arrogance are no longer necessary), then economic buffers and inequality are “inevitable” (pp. 116–20). In the anomalous case of the //Gana San, we find the combination Cashdan expects—food storage, property differences, some males successfully acquiring prestige and authority, payment

of substantial bride-price, and polygyny. Testart (1982a) is interested in the same variables as Cashdan but seeks to understand just the opposite—not the egalitarianism of nomadic foragers but the sedentism and socioeconomic inequality of food-storing foragers. Beginning with Steward’s (1968:328) seasonal-scarcity explanation of food storage in higher latitudes, Testart (1982a:528) hypothesizes a two-sided relationship between such storage and sedentism (food storage “inhibits the possibility of residential mobility” and “suppresses its necessity” [p. 524]) and an even more complex relationship between storage and social phenomena (all the “technical equipment . . . may . . . confer prestige” [p. 525], important people “will assume management of the stores,” making possible exploitation [p. 527], and “since the dissatisfied cannot leave so easily, exploitation can become more intensive” [p. 528]).

6. *The collective-hunting theory.* Steward (1936, 1955) points to the social organizational adjustment which foragers such as the eastern Athapaskans make to reliance upon large herds of migratory game. The 1936 paper mentions other possible causes of heterogeneous band composition, but the theory as to the effects of collective, seasonal musk-ox and caribou hunting can be recognized in its own right. Unrelated families come together in a “composite” group, resulting in bilateral descent and the possibility of band endogamy.

7. *The avoidance-of-social-disruption theory.* Turnbull (1968) offers a partially ecological explanation of the “flux . . . expressed as recurrent fission and fusion which . . . may be characteristic of the majority of [hunters and gatherers]” (p. 132). He argues that (day-by-day or seasonal) reconstitution of cooperating groups can serve as a systematic means for averting social disruption in band societies whose environments offer choices of subsistence techniques (pp. 135–37). He is neither the first nor the last to have portrayed foragers as coping in this way with latent or open antagonisms and aspirations to leadership within the community (e.g., Ehrenfels 1952:70–75; Gardner 1966b:396–97, 404; Arcand 1986; and see Mauss [and Beuchat] 1906), but the ecological element of his theory is original.

8. *The marketing theory.* Kroeber (1928, 1945), Jenness (1932), Steward (1936), Leacock (1954), Bose (1956), Fox (1969), and others see foragers as becoming simple culturally, exploiting resources familially or individually, and perhaps also becoming nomadic and bilateral as a direct or indirect result of involvement with external markets. The emphasis and completeness of the different versions of the theory vary considerably. Kroeber, for example, intuitively links trade, parasitism, and cultural simplicity. He speaks of Philippine Negritos (and other so-called primitive peoples enclaved within the *oikoumenê* of Eurasia and Africa) as trading forest products and developing cultural dependence or parasitism. Then, without any pretense at understanding or explaining the long-term processes which might account for the cultural meagerness of these dependent peoples, he asks us to recognize that theirs is probably a secondary simplicity (1928:19, 42–43; 1945:17–18): “To

2. Native Americans of California and the Northwest Coast are mentioned as exceptions.

infer, as is frequently done implicitly, that these peoples still are basal 'primitives' through having somehow remained such automatically for the last ten thousand years, completely insulated or in a geographical vacuum, seems contrary to the probability of our total experience of history" (1945:18). Jenness, Steward, Leacock, Bose, and Fox are more specific about the implications for foragers of becoming forest-produce suppliers in a market economy. Steward, building on particulars given by Jenness and others and arguing against Speck, contends that fur trapping in the North American Subarctic and possibly the fruit producing of the Semang led to "subdivision of band territory into family tracts" (1936:344). In summarizing North American ethnohistorical work on this subject both for and against the Jenness-Steward position, Leacock (1954:7) mentions a family's "important ties" as being "transferred from *within* the band to *without*" as its previously cooperative ties with other band members come to be "a positive hindrance" rather than a source of security. She goes on to note sources of Montagnais resistance to culture change as well as factors which serve to "ease and speed" change (1954:8-9). However, none of these contributing factors—including efficient steel traps, increasing scarcity of game, increasing importance of small fur-bearing game animals, and French preference for dealing with individuals—is an explicit part of her theory that private sale and exchange of furs "laid the basis for individually inherited rights to land" (p. 2). Bose contends that land pressure induced Birhor in forested Bihar and Orissa to give up (swidden) cultivation and specialize more in hunting small game and manufacturing rope for exchange, their resulting nomadism being "guided by the needs of trade" (1956:4-5). Fox, seemingly unaware of all the foregoing work, theorizes that five enclaved Indian and Sri Lankan foraging groups are specialized participants in larger systems. As he puts it (1969:142), "In such groups it is commonly observed that the single (nuclear) family functions as the prime economic unit. This situation seems to be a product of the individually competitive economic system where each family tries to maximize the amount of forest goods collected for external transactions." Fox judges "the equally prevalent pattern of highly migratory individuals, the lack of any formal kin pattern to the composition of settlements, and the lack of extensive reciprocity and sharing among family groups" to be "consonant" with such socially fragmented economics. Headland and Reid's (1989) recent survey is a forceful reminder of how many supposedly isolated peoples are "commercial foragers."

9. *The depopulation-displacement theory.* Hickerson (1960), who also considers the impact of external marketing, and then Service (1962) and Deetz (1968) tell us that spatial and social reorganization result from depopulation or displacement in contact settings. Hickerson's point is that it is inaccurate to portray the Chippewa as "endemic individualists" before the middle of the 17th century. The fur trade led to social "fragmentation" because some individuals migrated away from hunted-out regions, their new communities were neither auto-

nous nor "based on kinship or the possession of a discrete territory," and "by 1680 territorial occupancy was fluid and based on the contingencies of the moment rather than on tradition" (pp. 99, 100, 101). According to Service (1962:108), the composite band is "a product of the near-destruction of [virilocal, exogamous] aboriginal bands after contact with civilization. In all cases, there is conclusive evidence of rapid depopulation by disease which . . . resulted in the merging of previously unrelated peoples." He makes the parallel point that displacement may lead to the same unstructured or "composite" units (Service 1962:86, 88, 97, 101), and therefore his argument hinges more on the near-destruction of bands in contact settings than on the source of that destruction. It bears special emphasis that composite bands for him lack structure (p. 60). Deetz takes the same argument a step farther. Australians, the Gê, and Californians are unlike other foragers in two ways: they have lived for millennia in stable environments, "absolutely immune from influence and pressure" from food producers ("all other" foragers "having been more or less moved about and influenced by expanding Neolithic culture") (1968:283, 284), and they stand apart in their "strong development of moiety organization" (p. 284). Deetz doubts that this is a coincidence, suggesting that moieties may have done much to orient individuals and to integrate "thinly distributed populations" in the past but have proved too "delicate" to withstand the "frequent movements" and environmental changes which expanding food producers imposed (p. 284). Bender (1978:210-11) concurs with Deetz's main points, and Testart (1988:10-12) argues similarly with regard to Australians.

10. *A composite theory.* Balikci (1968) derives the socially "atomistic trends" of Vunta Kutchin from the disappearance of traditional moiety and leadership systems, the "weakness of the intrusive political organization established from above," and the introduction of "individualistic acquisitive techniques." While these trends provide a "fertile soil" for conflict between neighbors, co-workers, or companions, Balikci considers it "impossible to correlate the two precisely." For explanation of the interpersonal conflict, therefore, he proposes turning to the work of Freud and Simmel (p. 198).

11. *The subordination-dependence theory.* Gillin (1942), James (1961), and I (Gardner 1966b) have spoken of foragers' psychosocial responses to subordination and dependence in culture contact, and Orans (1965) puts forward a broader and more systematic theory about responses to encystment by a dominant society. We have drawn variously upon the work of Park, Horney, Merton, Hull, Merton and Rossi, and Kerckhoff and McCormick. Gillin and James regard slights and "status inferiority" in recent contact situations as contributing to Ojibwa anxiety, which "casts its shadow across the entire gamut of Ojibwa behavior" (James 1961:735) and which often results in withdrawal, distrust, and social disorganization. This is what others have termed social "atomism," or an emphasis on the individual. Surveying a nonrandom sample of 26 food-gathering societies, I

have found an association between chronic intercultural pressure (the long-term presence of more powerful neighbors) and an individualism described as a "recurring constellation of systematically interrelated cultural features" which includes nonauthoritarian parental roles, expectation of self-reliance, avoidance of overt aggression, gender and age egalitarianism, social control achieved by self-control, retreat from conflict, and supernatural sanctions, and individualized or memorate-level knowledge (1966b:400–410). I have pointed to several psychological processes as of possible explanatory value. In particular, the repression of hostility in the face of continual harassment might lead to anxious withdrawal, avoidance of either the display of anger or retaliation, the tendency to regard even advice as domination, avoidance of the envy or resentment of others, and the pursuit of self-sufficiency. Orans addresses the plight of "all encysted societies which have conceded rank to a dominant surrounding society" (1965:123). The "rank-concession syndrome" entails accepting social inferiority and relative powerlessness, adopting practices of the dominant society (so as to attain rank, get power, or comply with demands), and losing solidarity in the process (pp. 123–35). Orans compares the pursuit of rank through political power and through economic improvement and finds the latter path (that of foragers) "divisive because it encourages greater emulation on the part of those who have superior economic power. . . . Unlike success in the political rank path, which promises rewards to all, economic success is essentially individual. . . . all of the centrifugal tendencies inherent in rank concession and being encysted not only are free to run their course but are even accelerated" (p. 129). Gordon may be making a related point when he says that use of guns and "arbitrary violence" against the San may account for their "classless" (i.e., egalitarian) social order (1984:197, 200, 220).

12. *The domination-escape theory.* Though not elaborated into a formal theory, the suggestion that large power differences between foragers and their neighbors can shape foragers' social life certainly bears mention. As the argument goes, when foraging people remain mobile in order to escape domination by their neighbors, their residential instability affects their social structure. Recognition that marginal people can live elusively for protection is far from new. Robert Knox, for instance, described the silent trade of the Veddas in 1681, making quite clear its rationale (Seligmann and Seligmann 1911); many others since have appreciated the practicality of aloofness and flight from intergroup conflict (e.g., Gardner 1985; Sandbukt 1988:111). Miller and Dollard (1941:8–9) interpret Semang nonviolence as another such learned and "stabilized" response to the hostility of powerful neighbors. Dentan (1988) reiterates without endorsing this argument. He considers it possible that a number of societies and groups have accepted defeat, withdrawn into refuge areas, and reorganized "in adaptive ways" which include being flexible in group membership, egalitarian, and nonviolent.

Substantively speaking, the first seven theories all ac-

count for foragers' pursuit of individual autonomy in terms of what are said to be universal or particular aspects of their subsistence practices; the remaining five theories all center on the impact of frontier circumstances. Within these two substantively defined subsets only a few pairs of arguments need to be thought of as mutually contradictory. In the second subset, especially, there is striking complementation.

As we have seen, emphasis on individual autonomy can involve extreme individualism, egalitarianism, social structural simplicity and flexibility, and various combinations of these. In some of the theories other things have been mentioned, such as pressure for self-reliance, antiauthoritarianism and nonviolence, equal participation of the sexes in subsistence, social leveling mechanisms, and bilateral descent. There may be limited agreement that these characteristics should be added to the list, but most of them are clearly consistent with the main ones (e.g., self-reliance being an expression of individualism, antiauthoritarianism being a manifestation of egalitarianism, and so on). Considering all the additional characteristics as expressions, implications, etc., of individualism, egalitarianism, and social flexibility yields an overly simple but interesting perspective on the 12 theories treated (table 1). For instance, it brings out a variation in breadth of scope which is not generally recognized and a number of similarities between some of the subsistence and frontier-circumstance theories.

Most of the theories have a degree of plausibility, and few of them contradict each other. Whether they adequately represent aspects of the real world can be ascertained, however, only through empirical testing. All but three theories are at least 23 years old—quite old enough to have been scrutinized carefully. While several have their critics (e.g., those pertaining to avoidance of social disruption, marketing, depopulation-displacement, and subordination-dependence have been criticized by Pedersen and Waehle [1988], Hallowell

TABLE 1  
*Characteristics of Foraging Societies Dealt with in the 12 Theories Reviewed*

Theory	Characteristics
Adaptive child training	i
Nomadic food quest	i, e, f
Foraging mode of production	i, e, f
Resource depletion	e, f
Storage	e
Collective hunting	f
Avoidance of social disruption	f
Marketing	i, f
Depopulation-displacement	i, f
Composite	i
Subordination-dependence	i, e, f
Domination resistance	e, f

NOTE: i, individualism; e, egalitarianism; f, flexibility.

[1946], Eggan [1969], and Sandbukt [1988], respectively) and Woodburn (1988) has reexamined these same ones in general, only a few have been subjected to preliminary testing or to evaluation in terms of the others (e.g., theory 1 by its authors, theory 5 by Cashdan [1980] and Testart [1982a], and theories 2 and 9 by Ember and Ember [1972] and Ember [1975]). This is not to say that everyone else has ignored them. Bender (1978), Fox (1969), Hewlett and Cavalli-Sforza (1986), Meillassoux (1973), Morris (1977, 1982), Williams (1974), and Woodburn (1980, 1982), amongst others, have reiterated or expanded upon several of the theories. Yet the new contributors tend to tunnel along in well-established directions, seldom surfacing to evaluate empirically the relative worth of their respective lines of argument. What all this means is that the theories have yet to be fashioned into working instruments of science.

## Theory Complementation

A first step in reconsidering the overall picture is a summary of the theories in terms of their constituent arguments (fig. 1). As in any comparative exercise, compromises have been necessary in the areas of terminology and the inclusion of detail, but similar theories (or their constituents) have been merged in the summary only if the essential arguments can be subsumed by low-level generalizations. The greatest compromise of this kind is probably to be found in regard to Leacock and Lee's and Cashdan's lines of thought; while some of their basic ideas have, indeed, been incorporated, their concern with the way access, sharing, and sociability contribute to egalitarianism goes unmentioned. Among the many things excluded from attention are theorists' claims as to the proportion of foraging societies whose characteristics are being explained, their explicit or tacit claims that the factors they treat are, alone, sufficient to give rise to key features of these societies, and their responses to competing theories. These exclusions make a few of the competing theories look less mutually contradictory than their authors and proponents might wish, but they are necessary for now. First, difference in relative power or breadth of applicability is quite a separate matter from difference in substance, our immediate concern. While the former is important and must eventually be assessed, until the theories have been tested we are only dealing with claims about their power. As regards the second exclusion, theories are often simply phrased, giving us scant evidence as to whether their authors are open to the possibility that several factors give shape to foraging cultures. Compared with the overall breadth of the summary of figure 1, lists of factors by Lee and DeVore, Leacock and Lee, Testart, Steward, and Leacock are quite narrow. Finally, few theorists have responded thoroughly, objectively, or sensitively to each other's work, and it helps us little to review the responses we do encounter.

Even if the theories look less mutually contradictory here than some might expect, their substantive comple-

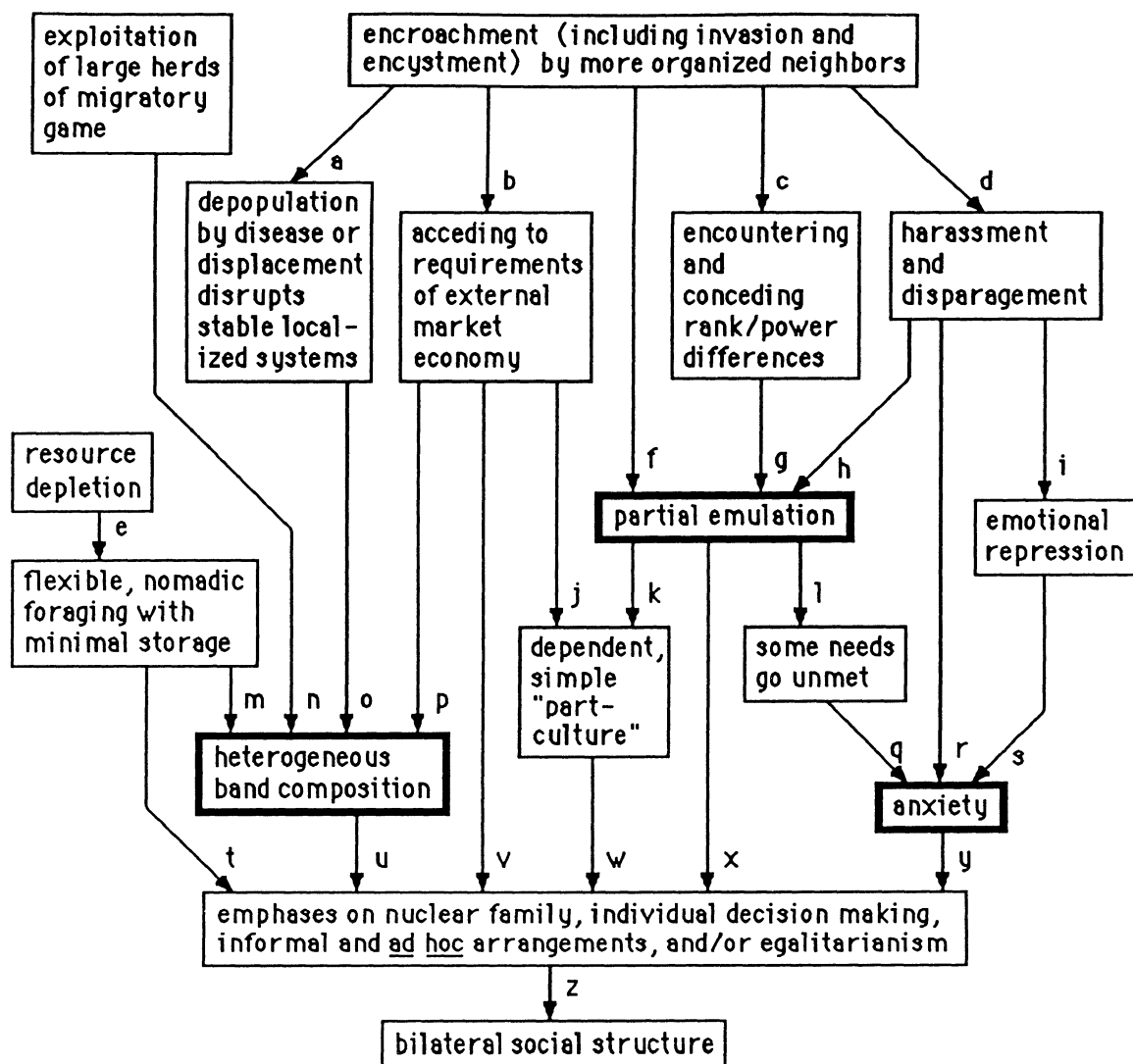
mentation remains clear. There are limits to it, of course. As a result of the generalizing by which the summary was produced, important constituents of five of the subsistence theories have been merged (see particularly *t*). But, collectively, these theories complement rather than contradict both the other subsistence theories and the frontier-circumstances theories; the latter, among themselves, also prove to be complementary. Complementation is a special kind of difference: reading across the row of boxes next to the top of figure 1, for example, we find key constituents of the depopulation-displacement theory, the marketing theory, and two variants of the subordination-dependence theory all standing separately, expressed in ways which do not preclude one another. These theories, after all, concern very different aspects of frontier circumstances which might all hold true in one place simultaneously. It is conceivable that, as a result of relatively new frontier circumstances, foragers who are already responsive to the demands of subsistence arrangements (such as those which figure in the subsistence theories just reviewed) feel the additional impact of demographic disruption, external marketing procedures, and humiliating subordination. Such multidetermination is not at all far-fetched. Various complementary processes could easily be working in parallel, and we should, perhaps, consider it *likely* that some of them will be doing so. Because of this, it is imperative that the theories be examined in relation to one another.

A second thing the summary brings out is the sharing of intervening variables by different theories. Lee and DeVore's nomadic-food-quest theory, Steward's collective-hunting theory, the Hickerson-Service-Deetz depopulation-displacement theory, and at least one version of the marketing theory all entail the idea that heterogeneous bands may be formed, with various further social implications (see arrows *m*, *n*, *o*, and *p*). In the same way, several theories have to do with emulation or anxiety and their respective consequences. These instances of convergent thought tell us that there are fewer lines of reasoning than there are theories. They require that we specify the nature and extent of differences between theories. Finally, they tell us that the respective fates of the theories, when finally tested, may prove to be linked.

## An Individual-Autonomy Syndrome

Among foragers and others who are described as pursuing individual autonomy, certain cultural features show up again and again: pressure on children for self-reliance, independence, and individual achievement; individual decision making in matters having to do with family, power, property, ritual, etc.; extreme egalitarianism, including extreme gender egalitarianism;<sup>3</sup> techniques for

3. Students of social stratification have repeatedly named certain foragers as being exceptionally egalitarian, including gender-



Barry et al 1959

t

Cashdan 1980

t

Deetz 1968

a

Foley 1988

e

Gardner 1966b

d h i s y

Gillin 1942

d l q r

Hickerson 1960

a o u

James 1961

c d g l q r y

Kroeber 1928

b f j k

Kroeber 1945

f k

Leacock 1954

b v

Leacock and Lee 1982

t

Lee and DeVore 1968

m t

Orans 1965

c d g h x

Service 1962

a o u z

Steward 1936

b n p u v z

Steward 1955

b n v

Testart 1982

t

Turnbull 1968

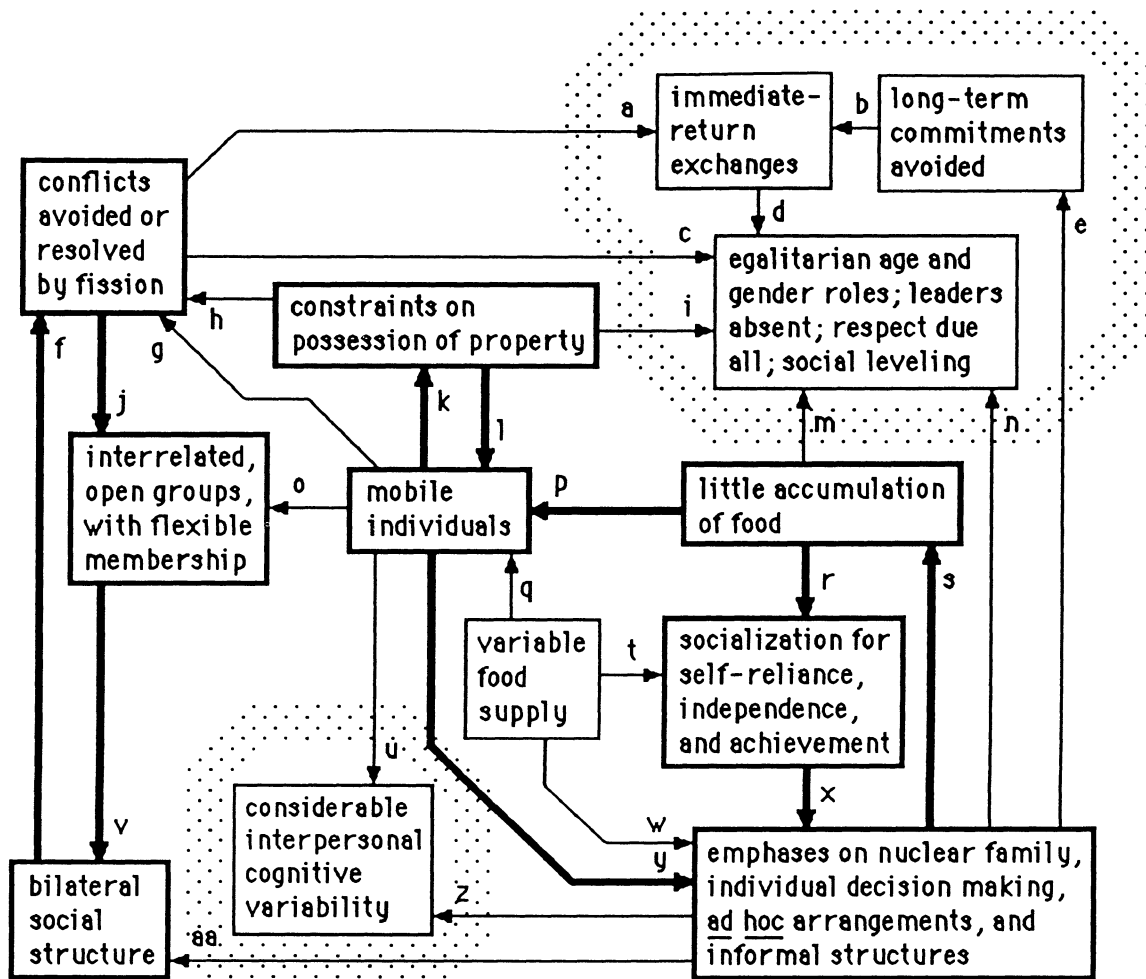
t

FIG. 1. Summary of leading theories of emphasis on individual autonomy among foragers.

prestige avoidance and social leveling; absence of leaders; what Meillassoux and Woodburn call instantaneous

egalitarian. In order of frequency of mention in nine such works, these are the !Kung, Mbuti, Paliyan, Eastern Hadza, Malaysian Negrito, and Montagnais-Naskapi (Gardner n.d.).

or immediate economic transactions; individual mobility and a corresponding openness and turnover in band membership; resolution of conflict through fission and mobility rather than by violence or appeal to authorities; bilateral social structure; a general tendency toward informal arrangements and individually generated,



Barry et al 1959

Cashdan 1980

Dentan 1988

Gardner 1966b

Lee and DeVore 1968

Lee 1972

r

i k q

j z

c g n u x y z

c g h i k l o q w

o q

r

Murdock 1949

Rohner 1975

Service 1962

Steward 1936

Testart 1982

Turnbull 1968

Woodburn 1982

o v

t x

y aa

aa

h i k m p

f g

a b c d e g i k l m o q

FIG. 2. Theorized relationships among features of foragers' individual-autonomy syndrome. Feedback loops are shown with bold lines; parts of the syndrome said to represent mainly output are set off with stippling.

*ad hoc* structures, and relatively high levels of interpersonal variability in concepts, beliefs, and manner of expression. This is far more than a list of coincidentally shared traits. Many of them can be recognized as probable manifestations of, means for, outcomes of, or (in some cases) perhaps preparation for individualized decision making. What is more, the theorists themselves propose that a number of the traits are intimately related to each other (fig. 2). The product of this second diagrammatic exercise is a theory in its own right: that many of the features commonly found in foraging societies

fit together in a veritable "individual-autonomy syndrome."

How might such a syndrome arise, and why do there appear to be numerous examples of it in the literature? Some structural principles (such as the ordering of behavior in terms of individual autonomy) are simpler than others. Their very simplicity should contribute to their being replicated (Vogt 1965) or extended widely through a cultural system if, for whatever reason, they are considered fundamental and pursued systematically. For instance, the principle under discussion is suscepti-



ble to such replication because it bears potential relevance to a variety of behavioral realms and semantic domains, it lacks complexities which could lead to structural disharmonies, and the associated values are readily expressed in compelling language (i.e., a child who is affected by a dispute may be said simply to have rights as an autonomous person, and so may both parties in a marriage or divorce arrangement). Eventually, it ought to be possible for the people in question to generate a many-sided but coherent and fairly consistent system. In this case, though, perhaps we should be speaking of the likelihood,<sup>4</sup> not possibility, of this outcome, right down to some of the details. Not only do there seem to be limited variant ways<sup>5</sup> of thoroughly institutionalizing an emphasis on anything as simple as individual autonomy but there is an apparent interlock among features of the system which goes far beyond replication of formal structures.

The intimate relationships which are claimed to exist among features of this syndrome, if actual, may help us to explain the reported extreme emphases of specific foraging societies on the individual (Lévi-Strauss 1961:310; Gardner 1966b:409), egalitarianism (see again n. 3), and "looseness" (Peltó 1968:40). Figure 2 shows that 8 of the 13 features are involved in positive feedback loops—loops which could drive many of the variables and perhaps the syndrome as a whole toward extreme forms.<sup>6</sup> Notably, although egalitarianism is not involved in any of the feedback loops, it is pushed by all of them.

One's labeling of a complex set of phenomena is always to some extent arbitrary, and disagreements are inevitable. Given the breadth of the syndrome, choice of a label for it will also be controversial if the term has been used in the past primarily for other, more specific referents. There are precedents for calling the syndrome

"individualistic" (Mead 1937, Gardner 1966b), "atomistic" (Honigsmann 1946, 1968; Rubel and Kuperer 1968), "loose" (Embree 1950, Peltó 1968), or "egalitarian" (Leacock 1978, Woodburn 1982). Although each of these terms is suitable in some way, each is problematic. Western ideological traditions complicate extending the terms "individualistic" and "egalitarian" to band-level societies (Dumont 1966, Morris 1978, Nelson and Olesen 1977), despite the ironic fact that foragers are better able and more likely to exhibit extreme, consistent, uncompromised individualism and egalitarianism than people of Western societies. "Egalitarian" is overly specific, too. "Loose" and "atomistic" have distracting, negative denotations. "Loose"—by which Peltó meant individualistic and elastic—conveys incorrectly the idea that both structure and behavioral controls are lacking. People in "loose" societies may, in fact, be highly self-disciplined. And "atomism," in either its social and economic sense or its psychological sense, suggests to an inappropriate extent a lack of connection between individuals. It conjures up a picture of a kind of system which is only occasionally encountered, one characterized by interpersonal repulsion and alienation; it gives no indication that one more often finds the members of an "atomistic" society exhibiting principled self-reliance within community settings.

Of all the features of the syndrome after which it might be named, individual autonomy may be one of the most appropriate. Individual autonomy implies much else in the syndrome; it bears on a great deal phenomenally—from decision making about subsistence resources to the expression of values.<sup>7</sup> As an anthropological label, it lacks specific and inappropriate prior referents other than, perhaps, Solway and Lee's (1990:120) use of the term for economic autonomy on a societal level. It even meets the approval of some recent ethnographers of foragers. Although such approval is an unnecessary luxury in comparative research, their reasons for approving deserve mention. Leacock has confessed a preference for the term "autonomy" over "egalitarianism" because the latter "confuses similarity with equity" (1978:247), and Myers identifies himself as one of several ethnographers trying recently "to fend off the assimilation of 'their' autonomy to 'our' individualism" (1988:276; see also Myers 1986).

## Testing the Theory of an Individual-Autonomy Syndrome

Because the *Atlas of World Cultures* (Murdock 1981) contains both a workable sample of foraging societies and coded information pertinent to five of the boxes (four of them particularly important ones) in figure 2, a

4. This is a bold claim, but it is not made without reason. Students of foraging cultures commonly take each other's findings for granted. We engage in comparison within our subsistence-specific samples without giving adequate thought to why we keep encountering certain cultural features. They can be written off all too easily as traits we expect among foragers. We may assume that we understand them even without having studied them in a controlled manner, and we may go farther to treat them erroneously as unique to foragers. It was startling to realize, as I read Oliver's (1965) account of the pastoral Kamba and then heard Bolton's (1984) presentation on Norwegian interpersonal relations, that I was anticipating much of the detail as the portraits were developed. When a number of familiar cultural traits showed up all together in (what were, for me) unexpected places, it jarred me into appreciating the extent to which they might fit together structurally and systematically. With each of these experiences, the logic of the syndrome became clearer, regardless of the mode of subsistence of those possessing it.

5. One dimension of seeming variation is the according of negative and positive value to others. The suspicious atomism reported for parts of the North American Subarctic (Hallowell 1946, James 1961) is extremely negative in tone by contrast with the dense, constant network of sharing and the institutionalized avoidance of unfairness attributed to the Batek and Jahai Negritos of Malaysia (Schebesta 1927; Endicott 1979, 1986).

6. We must thank Bateson (1935) for this insight about positive feedback and the progressive development of extreme forms.

7. Indeed, of the ten independent measures of inequality which Whyte (1978) isolated by cluster analysis, at least five are incompatible with general emphasis on individual autonomy.

preliminary test of parts of the theory is possible. The complementary findings of five previous cross-cultural studies can also be examined.

A hologeistic cross-cultural sample of foraging societies can be selected from the *Atlas of World Cultures* by defining foragers as peoples with 0–5% dependence on either animal husbandry or agriculture (cols. 7d and 7e, code o). In 37 of the world's 150 cultural provinces there is at least one such society, so that, using customary sampling techniques, it is possible to select a world sample of 37 foraging societies. When Murdock established a slightly larger set of sampling provinces in 1968, he tentatively selected the first society listed under each province for his standard world sample, his rationale being that it "is normally the one adjudged to be the best described" (1968:307); those same societies still, for the most part, head the province lists in the 1981 *Atlas*. Because the data gaps in some early and specialized forager ethnographies could substantially reduce the significance of conclusions to be drawn from our already small sample, the adequacy of each description is important. Accordingly, Murdock's 1968 procedure is followed here in selecting a society to represent each province. By major ethnographic region, that procedure yields the following sample: 3 from Africa (!Kung, Mbuti, and Dorobo), none from the Circum-Mediterranean, 5 from East Asia (Yukaghir, Gilyak, Vedda, Andamanese, and Semang), 3 from the Insular Pacific (Selung, Aranda, and Murngin), 19 from North America (Aleut, Copper Eskimo, Saulteaux, Kaska, Ingalik, Haida, Twana, Yurok, Pomo, Yokuts, Ute, Wadadika, Klamath, Sanpoil, Gros Ventre, Crow, Kiowa, Chiricahua, and Diegueño), and 7 from South America (Paraujano, Shiriana, Aweikoma, Bororo, Chamacoco, Tehuelche, and Yahgan).

The 13 components of the syndrome and their theorized relationships have been derived, of course, from the work of a number of scholars. While this gives breadth to the overall statement, many whose relatively specific ideas and findings went into the composite may find their materials summarized in unfamiliar terms. In addition, 3 of the components—bilateral social structure (which covers marital residence, descent, descent groups, and kin terminology), emphases on *ad hoc* individual and nuclear-familial structures, and egalitarianism—are especially broad in phenomenal scope. Because these components will all figure centrally in the theory testing, their breadth must be kept in mind from the outset.

Ten variables which are included in the *Atlas* are pertinent to five of the components of the individual-autonomy syndrome. By calculating the associations among these variables, a few of the relationships theorized to exist between components can be tested empirically. The five features and the *Atlas* columns which pertain to them are these:

*Mobile individuals:*

Settlement pattern (col. 30). As regards mobility, we can differentiate between people with "fully migratory or nomadic bands" (B) and those having more fixed settlement, including the seminomadic.

*Bilateral social structure:*

Marital residence (col. 16). Ambilocal (B) and neolocal (N) residence are pertinent to bilateral social structure; there are also some specific multilocal<sup>8</sup> residence arrangements (Uv, Vu, Pu) which resemble ambilocality in all but its balance. These are differentiated from unilocal residence or the nonestablishment of shared postmarital residence.

Cognatic kin groups (col. 24). Bilateral descent with reported kindreds (K) and bilateral descent with reported quasi-lineages based on filiation (Q) are pertinent, as is bilateral descent inferred from the absence of reported unilineal kin groups (B). This last possibility provides us with the information we might otherwise have obtained from cols. 20 and 22 as to absence of patrilineal and matrilineal kin groups and exogamy. Bilateral descent and kin groups are distinguished from unilineal and ambilineal descent and kin groups.

Kinship terminology for cousins (col. 27). The two bilateral patterns, Eskimo (E) and Hawaiian (H), can be differentiated from those associated with unilineality (Crow, Iroquois, Omaha, Sudanese, and descriptive) and those which are mixed.

*Emphases on nuclear family, individual decision making, etc.:*

Family organization (col. 4). Independent nuclear families, with monogamy (M) or with occasional or limited polygyny (N), constitute one expression of the familial and decision-making patterns described in the lower right box of figure 2. This kind of family organization is to be contrasted with the extended family in its various forms, including polygynous and polyandrous.

*Egalitarian age and gender roles; leaders absent; respect due all; social leveling:*

Jurisdictional hierarchy (col. 32). A minimal hierarchy, composed merely of independent families and autonomous communities (code 20), can be differentiated from multileveled, more complex structures (codes 21 to 44).

Class stratification (col. 67). Absence of significant class distinctions among freemen (O) is a clear measure of egalitarianism. It can be differentiated from both stratification and existence of wealth distinctions.

Succession to the office of local headman (col. 73). Absence of any such office (O) is certainly relevant. In addition, nonhereditary succession through informal consensus (C) covers many situations in which a so-called headman has a nominal or symbolic position with little real power. These can be distinguished from succession by heredity, seniority, influence, or formal means.

Inheritance of movable property (col. 76). Inheritance by children of either sex or both (C) expresses egalitarianism, as does the practice by which "a man's movable

8. After Ember and Ember (1972:382); see also Murdock (1981:94) with regard to ratios. "Multilocal" is an appropriate term for several combinations, but it will be used here only for the sets of choices which resemble ambilocal residence in all except frequencies.

property is destroyed or given away . . . or is otherwise not subject to any rule of inheritance" (O). These are differentiated from inheritance rules and practices which are unilineal or which evidence a bias toward sons.

*Immediate-return exchanges:*

Mode of marriage (col. 12). If, instead of bride-price, bride service, dowry, or two-way exchange of women between groups, there is "absence of any significant consideration, or giving of bridal gifts only" (O), we have one measure of avoidance of those complex, long-term relationships which Woodburn (1982) calls "delayed-return" exchanges.

The theory of an individual-autonomy syndrome states, amongst other things, (1) that mobility encourages development of bilateral social structure, emphasis on the nuclear family and individual decision making, and egalitarianism and (2) that emphasis on the nuclear family, in turn, encourages development of bilateral so-

cial structure, egalitarianism, and immediate-return exchanges. It is these selected aspects of the theory which can be tested against the facts by means of the *Atlas*. Figure 3 reports the  $\phi$  coefficients of association for the relationships which are being put to a test. The preliminary nature of this testing must be appreciated. The data fall far short of what would be needed for a full causal analysis; for that, the contributions of *all* variables in figure 2 would have to be taken into account.

Does mobility encourage bilateralism? The three tests of this reveal very low and, according to Fisher's Exact Test, statistically nonsignificant associations between being fully nomadic and having the residence rules, kin groups, and kin terminology diagnostic of bilateral social structure (nomadism and bilateral residence rule  $\phi = 0.07$ ,  $p = \text{n.s.}$ ; nomadism and bilateral descent and kin groups  $\phi = 0.15$ ,  $p = \text{n.s.}$ ; nomadism and bilateral kin terms for cousins [table 2]  $\phi = 0.20$ ,  $p = \text{n.s.}$ ). Despite their low magnitude, the associations are at least consis-

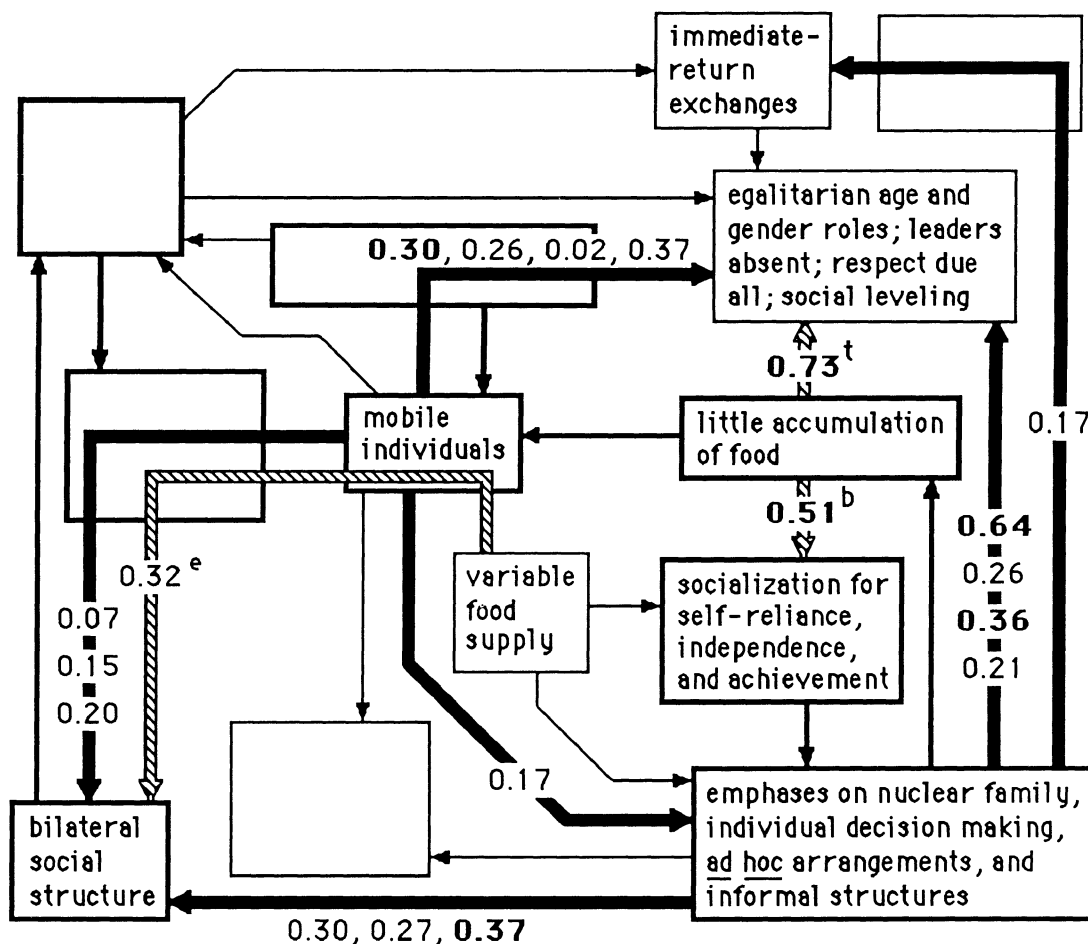


FIG. 3. Results of cross-cultural testing of some theorized relationships among components of foragers' individual-autonomy syndrome. Wide black arrows,  $\phi$  coefficients of 16 associations calculated using one foraging society from each possible cultural province in Murdock (1981) [N = 37]. Hatched arrows,  $\phi$  coefficients for associations studied previously by (t) Testart (1982a) [N = 40], (e) Ember (1975) [N = 32], and (b) Barry et al. (1959) [N = 79]. Boldface, associations which meet Fisher's Exact Test.

TABLE 2  
*Nomadism and Kin Terms for Cousins*

Kin Terms for Cousins	Settlement Pattern	
	Fully migratory or nomadic	Settled
Eskimo or Hawaiian	8	12
Crow, Iroquois, etc., or mixed	3	11
No data	2	1

$$\phi = 0.20, p = \text{n.s.}$$

TABLE 3  
*Nomadism and Jurisdictional Hierarchy*

Jurisdictional Hierarchy	Settlement Pattern	
	Fully migratory or nomadic	Settled
Minimal	9	9
With additional levels	4	15

$$\phi = 0.30, p = 0.05$$

TABLE 4  
*Nomadism and Class Stratification*

Class Stratification	Settlement Pattern	
	Fully migratory or nomadic	Settled
Absent among freemen	12	17
Stratification or wealth distinctions	1	7

$$\phi = 0.26, p = \text{n.s.}$$

TABLE 5  
*Nomadism and Inheritance of Movable Property*

Inheritance Practice	Settlement Pattern	
	Fully migratory or nomadic	Settled
No rule or by children of either sex or both	7	7
Unilineal or bias toward sons	2	11
No data	4	6

$$\phi = 0.37, p = \text{n.s.}$$

tent, and they are all positive. There is a similarly low, nonsignificant association between mobility and presence of independent nuclear families ( $\phi = 0.17$ ,  $p = \text{n.s.}$ ). Egalitarianism is a different matter. Three of the four variables pertinent to egalitarianism (tables 3–5) have moderately high associations with mobility ( $\phi = 0.30$ ,  $0.26$ , and  $0.37$ ), and the association between nomadism and minimal jurisdictional hierarchy is statistically significant ( $p = 0.05$ ). The one low association is that between nomadism and either a lack of headmanship or succession to such office by informal consensus ( $\phi = 0.02$ ,  $p = \text{n.s.}$ ). There are moderate positive associations between independent nuclear families and bilateral residence rule, kin groups, and kin terminology ( $\phi = 0.30$ ,  $0.27$ ,  $0.37$ ), the association with kin terminology being statistically significant ( $p = 0.03$ ) (tables 6–8). The association is low between independent nuclear families and the modes of marriage indicative of immediate-return exchanges ( $\phi = 0.17$ ,  $p = \text{n.s.}$ ). Finally, the  $\phi$  coefficients are all moderate to high for associations between independent nuclear families and the four measures of egalitarianism (tables 9–12); two of the latter are also statistically significant. These results suggest that emphasis on the nuclear family does indeed help shape the syndrome and does more toward this than mobility.

Barry, Child, and Bacon (1959), Rohner (1975), Ember and Ember (1972), Ember (1975), and Testart (1982a) offer complementary findings. They cannot be construed as fully independent tests of the theory, because the syndrome was identified with at least some of their findings in mind, but they do provide us with further cross-cultural documentation. Barry et al. use a world sample of 79 societies (of which 22 are foraging societies) to ascertain whether pressure on children for individual initiative and for obedience were related to low and high degrees, respectively, of accumulation of food resources. They report high and significant Yule coefficients in the two comparisons undertaken (1959:59). For the sake of comparability, their two comparisons have been combined into one, and this has been handled in the same statistical manner as the foregoing tests. For the association between extremely low accumulation of food resources and individual achievement and self-reliance,

the results are:  $\phi = 0.51$ ,  $p = 0.000005$ . Rohner (1975) makes a sufficiently similar point that it is not reported in figure 3. He contends that there has been special selection among foragers for child training (i.e., entailing parental acceptance) which produces people who are "self-confident . . . , self-reliant, and independent" (p. 115). Rohner does not give a full description or explanation of his statistical analyses, but 11 of the 13 foraging societies in his world sample are accepting or usually accepting of children, as compared with only 40 of the

TABLE 6  
*Independent Nuclear Families and Marital Residence*

Marital Residence	Family Organization	
	Independent nuclear	Extended, polyandrous, or polygynous
Ambilocal, neolocal, or multilocal	10	11
Unilocal or nonestablishment	3	13

$$\phi = 0.30, p = \text{n.s.}$$

TABLE 9  
*Independent Nuclear Families and Jurisdictional Hierarchy*

Jurisdictional Hierarchy	Family Organization	
	Independent nuclear	Extended, polyandrous, or polygynous
Minimal	12	6
With additional levels	1	18

$$\phi = 0.64, p = 0.0001$$

TABLE 7  
*Independent Nuclear Families and Bilateral Descent*

Descent and Kin Groups	Family Organization	
	Independent nuclear	Extended, polyandrous, or polygynous
Bilateral	11	14
Unilineal or ambilineal	2	10

$$\phi = 0.27, p = \text{n.s.}$$

TABLE 10  
*Independent Nuclear Families and Class Stratification*

Class Stratification	Family Organization	
	Independent nuclear	Extended, polyandrous, or polygynous
Absent among freemen	12	17
Stratification or wealth distinctions	1	7

$$\phi = 0.26, p = \text{n.s.}$$

TABLE 8  
*Independent Nuclear Families and Kin Terms for Cousins*

Kin Terms for Cousins	Family Organization	
	Independent nuclear	Extended, polyandrous, or polygynous
Eskimo or Hawaiian	10	10
Crow, Iroquois, etc., or mixed	2	12
No data	1	2

$$\phi = 0.37, p = 0.03$$

TABLE 11  
*Independent Nuclear Families and Succession to the Office of Local Headman*

Succession to Office	Family Organization	
	Independent nuclear	Extended, polyandrous, or polygynous
No office or by informal consensus	7	7
By heredity, seniority, influence, or formal means	3	15
No data	3	2

$$\phi = 0.36, p = 0.04$$

77 remaining. Although the  $\phi$  coefficient is not high, his sample size contributes to the significance of the finding ( $\phi = 0.23, p = 0.02$ ).

Ember (1975), employing a world sample of 50 foraging societies (data were available for 32 of these) and assuming that precipitation variability (i.e., variance/mean) is a measure of resource fluctuation, examines whether resource fluctuation makes bilocality more likely and concludes that it does. In order to express her findings comparably with the others reported in figure 3, her data

have been analyzed here in a less sophisticated way. In the reanalysis, the mean precipitation variability index is found to be 1.45 and there is a modest positive association between an index above the mean and either bilocal residence or unilocality with a prevalent alternative ( $\phi = 0.32, p = \text{n.s.}$ ).

Ember and Ember (1972:386) report a significant asso-

TABLE 12  
*Independent Nuclear Families and Inheritance of Movable Property*

Inheritance Practice	Family Organization	
	Independent nuclear	Extended, polyandrous, or polygynous
No rule or by children of either sex or both	6	8
Unilineal or bias toward sons	3	10
No data	4	6

$$\phi = 0.21, p = \text{n.s.}$$

ciation "between migratory bands and/or equality of inheritance (on the one hand) and multilocality (on the other)" ( $\phi = 0.21, p < 0.01$ ). Because they provide no basis for their figures, combine two variables in a way which is not fully explained, and deal with a relationship (i.e., egalitarianism  $\rightarrow$  bilaterality) which has no counterpart in figure 3, their finding is not shown in the figure. It does, however, help corroborate the individual-autonomy-syndrome theory.

Finally, Testart's (1982a:528–30) incompletely analyzed data on 40 foraging societies permit us to speak of two very significant associations, one between use (vs. nonuse) of food storage and presence (vs. absence) of social stratification ( $\phi = 0.73, p = 0.00002$ ) and the other between constancy of food resources and fully nomadic settlement ( $\phi = 0.45, p = 0.006$ ). The latter finding is quite contrary to the expectations of Lee and DeVore (1968), Lee (1972), Cashdan (1980), and Woodburn (1982). What are the implications of this? Variable food supply, in being largely environmentally defined, is unlike the other features of the individual-autonomy syndrome—more in the nature of input to the syndrome than an integral component of it. Therefore, what Testart's finding challenges is not the shape of the syndrome but one kind of explanation of the syndrome.

To summarize, the 19 associations reported in figure 3 and 2 of the 3 which are not so reported are positive. Although 2 of the  $\phi$  coefficients are near zero and 10 more are quite modest, 9 are between 0.30 and 0.73 and 8 are statistically significant. These associations provide more support for the idea of an individual-autonomy syndrome among foragers than we would expect by chance alone. Though not strongly supported, the theory certainly warrants closer scrutiny. The findings up to this point suggest that it would be worthwhile extending the data base to permit a full causal analysis.

The Venn diagram of figure 4 categorizes each of the 37 societies in terms of bilaterality, egalitarianism, and individualism. For purposes of this summary, societies have been called "bilateral" if they have all of the following: ambilocal, neolocal, or multilocal (see again n. 8) postmarital residence, bilateral descent with either

bilateral kin groups or an absence of unilineal kin groups, and (if data are available) Eskimo or Hawaiian kin terms for cousins. They have been called "egalitarian" if they have minimal jurisdictional hierarchy, absence of class distinctions, (if data are available) either no local headmanship or informal, consensual succession, and (given the data) inheritance of movable property by children of either or both sexes or disposal of property other than by inheritance. Because calling for all these traits may be overly restrictive, so long as there is no more than one data gap a society has been considered egalitarian if all but one of the criteria on which data are available are satisfied. Finally, societies have been termed "individualistic" if they meet the following two criteria fully or one criterion fully and the other by way of the specified alternative: independent nuclear families (or, alternatively, independent polygynous or polyandrous families) and bridal gifts only at marriage (or, alternatively, token bride-price or bride service)—both of these being arrangements which offer people relative independence of other kin. The results of this exercise are that only 8 societies out of 37 are classed as bilateral, egalitarian, and individualistic, and 13 societies are their opposite. This demonstrates well the limited distribution of the apparent syndrome in its full form.

### Testing Whether Pursuit of Individual Autonomy is Multidetermined

In reflecting on the theory complementation which is evident in figure 1, I suggested that several complementary processes might occur simultaneously in one place. Adequate treatment of the idea requires that we consider several things. The individual-autonomy syndrome involves diverse phenomena, and the pursuit of individual autonomy takes sufficiently extreme forms in some foraging cultures that its explanation is unlikely to be simple. Foragers' environments are complex, and the factors which might plausibly help shape the syndrome are numerous and varied. Finally, few of the theorized independent variables are mutually contradictory. Clearly, a wide perspective must be adopted. Although we need to weigh as many of the theories as possible (which could lead to excluding some of them from further consideration), that is only the first step. The exercise will not simply end with comparison of the explanatory power of the theories (as in Hunt's [1978] "cascade model" for excluding improbable explanations).

If it makes empirical sense to speak of a phenomenally complex individual-autonomy syndrome, if the several features of the syndrome may be shaped by diverse environmental factors (and we have some testable theories which spell out a few of the possibilities), and if those features are interrelated (as testing of the theory has just suggested) so that an impact on one of them has ramifications within the syndrome, then there will be multidetermination of the syndrome, and study of this re-

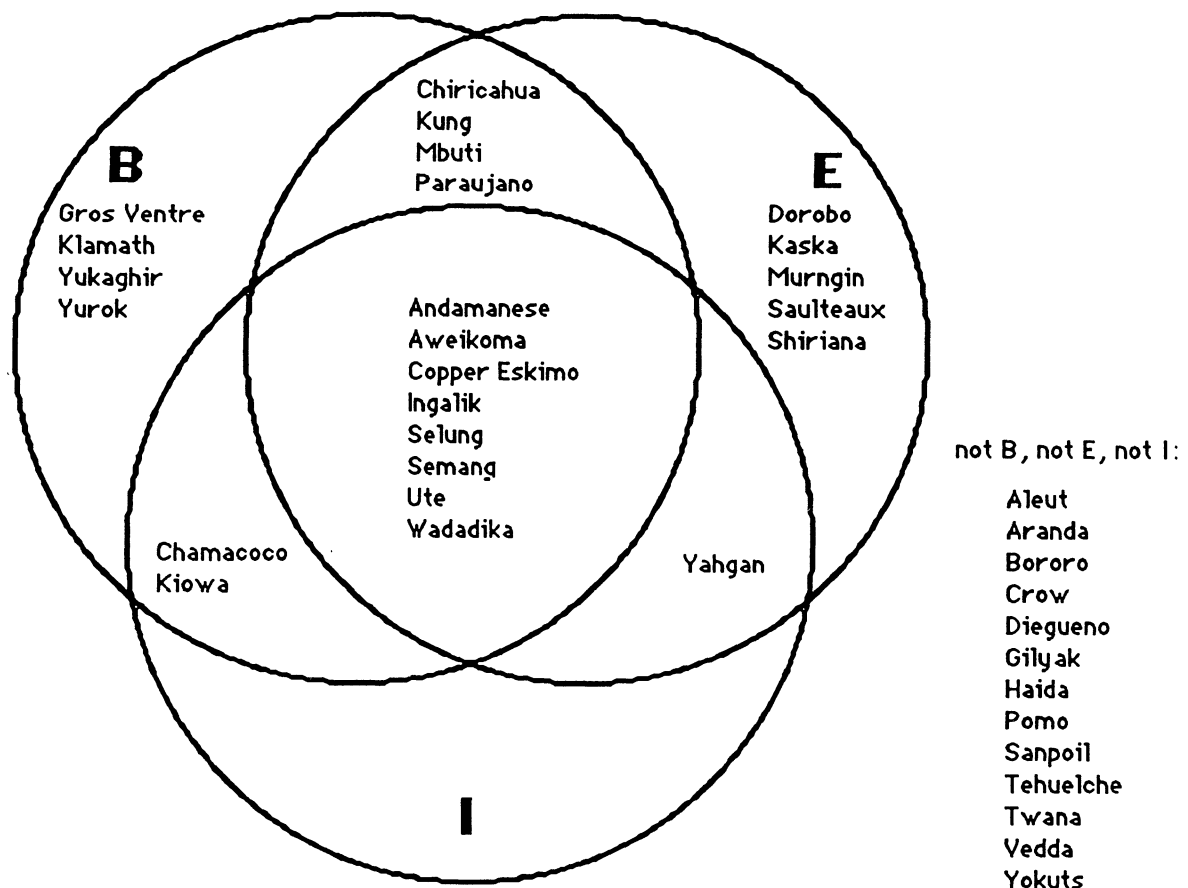


FIG. 4. Summary of the bilaterality (B), egalitarianism (E), and individualism (I), according to criteria from Murdock (1981), of the 37 societies examined.

quires no special justification. Earlier tests of four of the preexisting theories and reflections on two others give us a way of starting. The adaptive-child-training theory has been tested carefully and shown twice to be well supported by cross-cultural data (Barry et al. 1959, Rohner 1975), but we should take note, at least, of one earlier disconfirming study. In the course of their investigation of child training and personality, Whiting and Child (1953:338–43) rated the initial indulgence of infants, the severity of child training in general, and the severity of independence training for a holoecistic sample of 75 societies, 16 of which were foraging ones. Unexpectedly (from the viewpoint of Barry et al. or Rohner), in all three areas the mean ratings given foragers and nonforagers were virtually identical. But, because the two later studies address the theory directly and because they utilize more recent sampling methods, their positive, statistically significant findings will be deemed more meaningful than the patterns now found in Whiting and Child's data summary table.

Three component ideas of the nomadic-food-quest theory have been put to a test by Ember and Ember (1972) and Ember (1975). They examined the extent to which mobility, variable resources, and small size of local groups contribute to multilocality. The first test

demonstrated a weak but significant association between mobility and multilocality for a world sample of 688 noncommercial societies ( $\phi = 0.16$ ,  $p < 0.01$ ) (Ember and Ember 1972:383–84). Nonetheless, when depopulation was controlled for, there was no apparent independent effect of migratory settlement pattern on multilocality (1972:391). Ember's examination of the importance of variable resources (1975) has already been described. She also studied whether, in part because of the likelihood of unequal sex ratios, small local groups are associated with flexible postmarital residence arrangements (1975). (This is a detail of Lee's theory which was not included in the summary given earlier.) Again, she found a modest association.

Ember and Ember (1972) and Ember (1975) have also tested the depopulation-displacement theory. In the 1972 study, strong, significant associations were found between recent depopulation and multilocality in two world samples of noncommercial societies ( $\phi = 0.42$ ,  $p = 0.02$ ;  $\phi = 0.56$ ,  $p = 0.006$ ) but not in a subsample composed of societies with migratory bands (1972:387–91, 393). When Ember drew a new and larger sample of foragers and refined the conceptual distinctions, a significant association was found: she reported  $p < 0.017$  by the Mann-Whitney  $U$  test; reanalysis here of

her table 8 yields  $\phi = 0.44$ ,  $p = 0.05$ , by Fisher's Exact Test (1975:213–15). These figures are enough to establish depopulation as a factor in the development of bilateral social structure; they may not be enough to justify Ember's conclusion that "many if not most of our recent cases of bilocality are probably recent developments" (p. 221). We should be cautious for several reasons: (a) Ember identifies depopulation very differently from Service; (b) 34 of the 54 foraging societies in Murdock's 1949 world sample are bilateral, and the long-term social structural prehistory of a high percentage of them (reconstructed on the basis of internal evidence) is either Hawaiian or Eskimo (Murdock 1949:332–46); (c) ethnohistorical work on regions such as the Subarctic shows possible previous widespread distribution of bilaterality (Helm 1968, Rogers and Smith 1981, Smith 1981); and (d), so as to appear socially proper to their pushy neighbors in contact situations, enclaved people including foragers often adopt a veneer of borrowed unilineality which can be mistaken for a residue of past practice (von Fürer-Haimendorf 1943:283–84; Turnbull 1965:27–28; Gardner 1966a; 1972:426–28; 1985:418–19; 1988; Dentan 1988:278).

Elements of both versions of the storage theory have stood up to initial examination. First, Cashdan's unbuffered-environmental-variability thesis was supported, originally, by informal comparison of two societies. While the comparison itself fell short of being "controlled"<sup>9</sup>—and short of what might justify her speaking of "obvious" reasons (1980:116) or "inevitable" results (pp. 119, 120)—the exercise as a whole was thoughtful and sensibly argued. What is more, because Cashdan focused on matters which are central to the nomadic-food-quest and resource-depletion theories as well, her theory does not have to stand up to testing wholly on its own. Second, as we have seen, Testart's theory as to the relationship between food storage and inequality has been corroborated.

Though still untested, two other theories warrant brief discussion. Time and further archaeological inquiry are needed for full, adequate empirical testing of the resource-depletion theory. There may already be consensus, though, on one of Foley's main points. What he says about post-Pleistocene foragers' finding themselves in changed circumstances is widely appreciated; this much has been said before, even by those Foley criticizes (e.g., Lee and DeVore 1968:5). Nevertheless, we have a lot to learn about those changes and their implications. We must bear in mind especially that environments, strategies, and trajectories of change were diverse both in the distant past and during the Holocene (Bender 1978, Ember 1978, Foley 1988). Intensification has now, for example, been documented as taking place in different forms, at different times, and in diverse parts of the Old and New Worlds, including Australia (Price and Brown 1985), and where it occurred resource depletion

itself has been anything but uniform. Parkington (1984:167–71) reconstructs a late Holocene sequence for the southwestern Cape of South Africa, where newly arrived herders apparently forced foragers to leave the coastal plain and focus on "rugged parts of the landscape" only some 1,800 years ago. This resembles, in all but immediate cause and timing, the sequence Foley lays out for the Holocene in general. In other words, the San appear to have been pushed relatively late into adoption of the theorized post-Pleistocene strategy. But then Foley does not tell us that the changes occurred everywhere in synchrony.

My own contribution to subordination-dependence theory (flawed by hologeistic but nonrandom sampling) reports an association between foragers' being under chronic pressure from more powerful neighbors and manifesting several traits such as self-reliance and egalitarianism (Gardner 1966b). Is there such an association? Recent students of social stratification act as if there were. They top their lists of strikingly egalitarian peoples with six foraging societies, five of which are enclaved and pressed by neighbors in Africa or Asia. Their display of consensus is best viewed not as corroboration of the theory but as a sign that we need to test this and all the other theories which bear upon culture contact.

In the case of the nomadic-food-quest theory, besides what the Embers have ascertained about multilocality, we can test Lee and DeVore's (1968) idea that foragers' egalitarianism is shaped by their nomadism. Tables 3–5 suggest that it may be so shaped. And, looking at the summary classification of societies in figure 4 in relation to *Atlas* data on settlement pattern, we get a similar overview, there being a moderate, significant association between nomadism and overall classification of a society as egalitarian ( $\phi = 0.30$ ,  $p = 0.05$ ).

With four theories standing up fairly well to tests and others showing signs of promise, we have adequate reason to continue the inquiry. Regardless of the magnitude of their individual effects, if even a few factors do act in concert to shape and extend some foragers' emphasis on individual autonomy, those factors are doing what Moore and Fine call "multidetermining" the results (1968:69). "Overdetermining" is a common near synonym (Moore and Fine 1968:69; Foulks 1972:1–2; Rabinow 1977:150; D'Andrade 1984:98). While the latter term has been employed at times misleadingly,<sup>10</sup> important contributions have been made by some who use it. Foulks employs the idea in his attempts to understand, in ten individual patients' cases, which biochemical,

10. This term, taken from geometry, is used in medicine to refer to multiple causation or multiple determination. As Moore and Fine put it, in geometry, "two intersecting lines determine a point; three lines intersecting at a point overdetermine the point" (1968:69). There are slight disagreements on how to apply "overdetermination" in the study of physiology, psychiatry, and cultural anthropology—the problem being that it has purely formal meaning in geometry and is used to refer to cause-effect relations in the other fields. Some users of the term do agree, however, that it does not imply that there are more contributing factors than necessary. Moore and Fine consider this misleading and so prefer the term "multidetermination" instead.

9. It cannot be compared with the intraregional comparisons, each of several societies, by Nadel, Eggen, Edgerton, and even Durkheim, done in ways which allowed much to be held constant during the controlled examination of particular variables.



psychological, and sociocultural factors in the etiology of Arctic hysteria played a precipitating role and which contributed without being major determinants. He develops an effective, synthetic approach which helps him to understand both the general phenomenon and the diverse cases. D'Andrade uses the idea of multiple determination more casually, with a view to appreciating the intensity of social sanctions. Of one system he says that "there are conformity pressures of many kinds. . . . Perhaps what is surprising is that anyone can resist the directive force of such a system" (1984:98).

The approaches of Foulks and D'Andrade could in the long run both be made pertinent to particular aspects of the multidetermination of the individual-autonomy syndrome. Foragers, with their diverse natural settings and contact histories, should vary from society to society in the precise subsets of factors contributing to their seeking individual autonomy and in the weighting of those factors. Study of each possible causal factor, then one-by-one study of the societies along the lines of what Foulks did with patients may be what is needed for ascertaining with any certainty which factors are most important in shaping each case. As regards D'Andrade's line of thought, the extremes for which some foragers are noted demand explanation too. If multiple causal factors really do act in concert (and in addition to the possible intrasystemic positive feedback loops discovered above), exaggerated development of resulting forms is only to be expected. Establishing the extent to which they act in this fashion would require, again, a synthetic approach.

## Conclusions

The preliminary testing of the proposed individual-autonomy-syndrome and multidetermination theories has yielded relatively modest results. Indeed, definitive disposal of both theories is beyond the scope of this exercise; what has been achieved has less to do with corroboration and rejection than with establishing the need for extending a recently developed analytical perspective to a new subject area. The perspective seems applicable and promising. Enough has been shown for us to deem it a mistake to suggest that the character of foragers must be explicable in terms of one or another of the several most prominent theories of the moment. It is evidently also a mistake to characterize or typify foragers in terms of one pattern. For instance, the individual-autonomy syndrome appears to have a limited distribution; by the measures used here, only 22% of the sample societies possess it and a mere 19% more exhibit two of its three main aspects. The stance adopted here may be an aloof one, but the hope is that it will free us from a long-standing conceptual impasse,<sup>11</sup> permitting us to direct our efforts more productively.

A full causal analysis of the individual-autonomy syndrome should be undertaken as a prelude to further testing of the multidetermination theory. Further, even if the individual-autonomy syndrome turns out to be empirically valid, the theories which led us to it all need to be scrutinized critically. Testart's findings on variable food supply and mobility remind us that, although many of the 12 theories reviewed have received a measure of support, we have to be prepared to rethink the best-argued elements of each of them. Finally, an anthropologist, Gregory Bateson, was among the pioneers in recognizing the systemic implications of positive feedback (1935). Given the anthropological predilection for studying extreme cases (e.g., egalitarianism, cognitive diversity, potlatching, nonviolence, violence, the Nayar family, and so on), there is irony in our having for so long disregarded the potential explanatory power of his contribution. Its importance to students of foraging cultures is particularly clear.

## Comments

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It is unusual for anyone in hunter-gatherer studies to advocate a perspective which takes into account a number of alternative theoretical models, much less try to put them all together. Gardner's enjoyable article comprises both a worthwhile overview of explanations of individual autonomy and a new explanation based on multiple, complementary, and interrelated causation. It also offers some new insights into some old ideas, and this is at least as valuable.

A few things which stand out among the comparisons Gardner makes are (1) the diversity of perspectives found in hunter-gatherer studies, (2) the fact that some theories group anthropologists not previously thought of as holding the same views, and (3) the atypicality of Australian Aboriginal society from several different perspectives. With regard to the first point, Gardner by no means covers all perspectives in hunter-gatherer studies. He gives primacy to those which help to explain the establishment and maintenance of individual autonomy and not those which try to explain other differences among hunter-gatherers, such as climate, habitat, or diet—all major concerns in the 1960s. With regard to the second point, Gardner's delineation of the theories makes for some interesting insights and some groupings of perspectives sometimes seen as alternatives. His category "storage theory," for example, includes the positions of both Cashdan (who emphasises the practice of sharing, i.e., the failure to store goods, as a cause of egalitarianism) and Testart (who emphasises storage as a

11. Our speaking for and against a set of theories for more than 20 years without subjecting them to proper testing is, at the very least, indicative of a refusal to confront the issues.

cause of inequality). With regard to the third point, Australian Aborigines are unusual perhaps within adaptive-child-training theory (as Whiting [1968:337] suggests) and in nomadic-food-quest theory but even more so in several of the more specific theories. This is notable in the case of storage theory in the sense that rights in people might be regarded as "storable" or, to use Woodburn's (1980:108–9) somewhat exaggerated phrase, the Aboriginal women may be "farmed out" by older men.

Of the eight theories Gardner regards as relatively specific, storage theory seems to hold the most promise for a general understanding of causal mechanisms of hunter-gatherer social organization. In my view, this is partly because it is itself multidimensional, particularly in the conception of storage theory proposed by Woodburn (1980, 1982, 1988). Woodburn's "delayed return" category is essentially characterized by storage, which, as Testart (1982a, b) shows, is probably the most significant factor in the origin and development of social inequalities. In a sense, Gardner's individual-autonomy theory represents, for the *forager* rather than the anthropologist, one perception of how society should work. Storage theory, again from the forager's point of view, is another. It entails alternative aspects of individualism (in the choice to build up rather than share the surplus), if not egalitarianism. I prefer to think of the former as often comprising a *foraging mode of thought*, which ideally involves an immediate-return economic strategy, a pursuit of autonomy in social relations (e.g., choosing one's "kin" through gift exchanges in lieu of maintaining kin-group structures), and even a fluidity in cosmology comparable to the fluidity in social relations (see, e.g., Barnard 1988).

In sum, Gardner's article is for me not so much a new conception of foraging society as an opportunity to look at some old ideas in order to understand better the complex relations between them.

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Gardner points out that scholars have examined a range of features characteristic of hunter-gatherers as more or less independent and suggests treating them as interrelated. This is to be welcomed. The term he suggests—the individual-autonomy syndrome—is satisfactory. Moreover, it is possible to glean from his paper useful advice on how to explore this syndrome. We must not expect one factor to explain it or expect it to be identical in all forager societies. Nor should we confine our attention to foragers, for this syndrome may also be found among non-foragers; indeed, it need not be related to mode of subsistence at all but may be sustained by various "positive feedbacks."

While Gardner's idea is useful, his methodology and conclusions do not do it justice. I have serious reserva-

tions about his metatheoretical approach. He presents 12 "theories" comprising part-arguments from general theoretical works together with three-line introductory comments, narrow ethnographically based propositions, and non-specialist speculations, all taken out of historical, ethnographic, and discursive context. He analyses these so-called theories as if they were empirical phenomena of some sort (symbolic designs?). He compares and adds, condenses and reduces, correlates and cross-references them; he re-presents them in graphic form and looks at their flows, where they meet and where they separate, and cross-checks his analyses. Computer users know that not even the most sophisticated techniques can alter the quality of the input ("garbage in, garbage out"). Gardner's conclusions—"some foraging cultures possess an individual-autonomy syndrome" and "foragers' pursuit of individual autonomy is 'multidetermined'"—offer at best a minor contribution to the understanding of the syndrome he has identified. The test of these conclusions by correlation analyses based on 37 "hunter-gatherers" from Murdock's *Atlas of World Cultures* is also inadequate. Gardner takes data uncritically from the *Atlas*, ignoring the question of who is and who is not a hunter-gatherer, the breadth and depth of the ethnographic work on which a given entry is based, and recent revisions of the ethnographic work. The !Kung and the Mbuti, for example, cannot be included axiomatically in the category of "peoples with 0–5% dependence on either animal husbandry or agriculture."

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Gardner is to be commended for integrating many disparate variables into a single theory and for pointing out some hitherto unnoticed connections between the specific theories from which he draws. The variables comprising the individual-autonomy syndrome are often mixed together in a verbal stew when ethnographers talk about foraging societies, and it is important to consider whether and why they are related. Gardner's empirical investigation is a valuable contribution toward the former, and his figure 2 is a beginning on the latter. Here, however, we need more work.

It is easy to fool ourselves into thinking that variables are logically related when we know that they are empirically often found together. In my view, the logic of the relationships Gardner discusses (figures 2 and 3) is not adequately argued, either by Gardner himself or by many of the theorists he cites. For example, extreme individualism and egalitarianism are two key variables in the individual-autonomy syndrome. Certainly, they are often found together among foragers. Yet the logic of their relationship is not clear. One might argue, instead, that capitalism, with its associated *lack* of egalitarianism, leads to the greatest individual autonomy.

The economic leveling of a communist society, particularly where the leveling is implemented by norms of sharing, could logically be expected to impose severe constraints on individual autonomy. Perhaps these variables are often found together among band-living foragers because they are consequences of two very different aspects of the foraging economy. Perhaps autonomy with regard to wealth accumulation has nothing to do with autonomy in other areas.

Similarly, the factors said to be associated with the individual-autonomy syndrome are not always obviously related and at least in one case contradict each other. The "marketing theory" of Leacock and others is said to be a way of explaining, among other things, individually inherited rights to land. I suppose that individual landownership could be seen as a manifestation of individualism, part of the individual-autonomy syndrome. However, a flexible residential and territorial organization, probably associated with the *absence* of individual landownership, is also taken to be an attribute of the individual-autonomy syndrome. The syndrome begins to seem rather slippery.

Because "autonomy" is an attribute of individual personality and behavior, I think it is particularly important to specify and explain its relationship to socioeconomic variables such as foraging economy, egalitarianism, nomadism, etc. Why should they be related? How are they related, if they are? Presumably, child training (Barry, Child, and Bacon 1959) is the proximate mechanism through which individuals are trained to behave in ways that are adaptive in particular socioeconomic circumstances. How does this work? Draper and I (1988) have suggested that a shift from "parent rearing" among the mobile San to something approaching "peer rearing" among more settled, food-producing San may be a vehicle for these changes. Children in the settled camps had a greater percentage of their interactions with peers, as opposed to adults, than did children in the mobile foraging camps. Although there was apparently no conscious socialization for obedience and responsibility by parents in the settled camps (perhaps because of the recency of the subsistence shift), the socialization experiences of children differed drastically simply as a result of the subsistence shift itself. Behavior related to obedience (compliance with adult requests) showed an increase in girls in the settled camps, as one would expect, although the reason seems to be related less to conscious socialization than to an increase in sex-typed work by children.

Finally, the finding that constancy of food resources is associated with "fully nomadic settlement" is not surprising, but a lot depends on how these variables are measured. It is probably not useful to think of mobility *per se* on an ordinal scale (see Binford 1980 on the distinction in mobility strategies between "foragers" and "collectors"). Do the !Kung have "fully nomadic settlement"? Groups in the comparatively "constant" tropical forests, such as the Ache, are far more mobile, in terms of *miles per year*, than the !Kung, yet the *interregional visiting* of the latter is a mobility response to the absence of constancy in the Kalahari environment.

This is an interesting paper, although its ambitious scope means that there will surely be something for everyone to argue with. The individual-autonomy syndrome is not without problems, but it may have more analytical value than the category "forager."

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Gardner is correct that his study has "yielded relatively modest results," but that is not surprising given the apparent flaccidity of the idea that hunter-gatherer societies are typified by an emphasis on individual autonomy. A plurality of 35% of the societies in Gardner's sample have none of the three characteristics he uses as markers of the individual-autonomy syndrome (bilaterality, egalitarianism, and individualism), while 25% have only one of them and only 22% have all three. Therefore a better question might be why anthropologists have persisted in believing that hunter-gatherers are typically strongly individualistic and egalitarian. While I have no doubt that hunter-gatherer societies tend to be relatively individualistic and egalitarian compared with those of food-producers, I suspect that the staying power of this image of hunter-gatherer societies in the anthropological mind and the corresponding failure of anthropologists to deal adequately with the diversity shown by foraging societies may have as much to do with the characteristics of the societies that produce anthropologists as with the characteristics of hunter-gatherer societies themselves. More so than for the other types of societies anthropologists study, the abstract notion of hunter-gatherer society is both emotionally and ideologically powerful both for us anthropologists and for our audiences, and our interpretations of such societies should be examined with an awareness of the societies at which the interpretations are aimed.

I am also bothered by a problem in a cross-cultural methodology that is by no means confined to this article: it is unclear whether what is being coded is behavior or simply informants' statements about behavior. Given the frequently observed lack of fit between what informants say they do, what they say they ought to do, and what they do, this is not a trivial issue (see Cancian 1975; Cronk 1991a, b; Harpending and Draper 1986; Harpending, Draper, and Rogers 1987). Is individual autonomy simply an ideology, or is it a reflection of behavioral reality? Much of what gets coded into the ethnographic data bases is informants' strongest and most emphatic statements—statements that may well be better seen as manipulative exhortations than as accurate reflections of behavioral reality (Harpending, Draper, and Rogers 1987:138). Thus it is precisely in those societies in which an ideology of individual autonomy and egalitarianism is most loudly and enthusiastically proclaimed that real individual autonomy and egalitarian conditions may in fact be most threatened. Given that most of the information coded in the cross-cultural data

bases on the societies in Gardner's sample was obtained either after hunting and gathering had been largely abandoned or during periods of rapid change, it is quite possible that many of the statements recorded by ethnographers about these societies had much to do with attempts to manipulate and control volatile social and economic situations and little to do with actual behavior. Until cross-cultural studies include detailed examinations of the types and quality of data in the original sources, I will remain skeptical about their usefulness or relevance to the hypotheses being tested.

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Gardner's work is always clear and original, combining holism with meticulous attention to detail. This article is typically thought-provoking.

One test of the individual-autonomy syndrome is to select a sample using a criterion other than foraging. As it happens, an independently conceived, nonhologistic and nonrandom sample based on the criterion of peaceability reveals a formally similar syndrome (Dentan n.d.a., b), confirming that the individual-autonomy syndrome and multidetermination of it are social facts. It also suggests a modification to the syndrome. The peaceability sample includes cenobites, Gibson's (1986) "Type 1" swidders, Alcoholics Anonymous, and the Rainbow Nation (Niman n.d.). Thus foraging, although conducive to it, may not be necessary or sufficient to cause it. These groups do tend to share a fission-fusion demographic structure (e.g., Fix 1975, 1977), which can be an adaptation to variable resources but also typifies organisms under locally intense predation—for example, for humans, slaving ("domination escape theory"), disease, or ecocide ("depopulation theory"). These factors affect nonforagers as well as foragers.

This observation suggests some weaknesses in the hologistic test:

1. Perhaps one reason for low correlations is that the criterion of sample selection (foraging) is only indirectly related to the syndrome and thus relatively minor in determining it. The longer the causal chain, the lower the correlation between cause and effect (Ellen 1982). Gardner's sample may be inappropriate to the thrust of his study.

2. Since the individual-autonomy syndrome is a system, some elements (causes) change at different times from others (effects). The ahistoricity of the *Ethnographic Atlas* prevents discriminating between causes and effects and thus demonstrating causal connections. A historical study which included nonanthropological sources would complement Gardner's work.

3. The decontextualized coding of the sample is armchair anthropology. In real life, succession by "informal consensus" does not exclude "seniority, influence." Similarly, Gardner notes that Sub-Arctic atomism differs in emotional tone from Semang individualism.

Emotional tone is useful to ethnographers but not to coders. Most emotions are ambivalent: joking and respect relationships go together like ham and eggs; so do homosexuality and homophobia; even Semang ties involve some anxiety. These ambivalences escape +/− coding. Intensive ethnographic study is imperative. I plan to take Gardner's paper into the field to guide my own work.

4. Distinguishing yuppie egotism from Semang individualism may be useful. The brilliant 19th-century social observer Alexis de Tocqueville, who coined the term "individualism," writes (1965 [1840]):

Egotism is a passionate and exaggerated love of self, which leads a man to connect everything with his own person, and to prefer himself to everything in the world. Individualism is a mature and calm feeling, which disposes each member of a community to sever himself from the mass of his fellow-creatures; and to draw apart with his family and his friends; so that, after he has thus formed a little circle of his own, he willingly leaves society at large to itself. . . . Egotism blights the germ of all virtue; individualism, at first, only saps the virtues of public life; but . . . is at length absorbed in downright egotism. Egotism is a vice as old as the world . . . ; individualism is of democratic origin, and it threatens to spread in the same ratio as the equality of conditions. . . . Thus not only does democracy make every man forget his ancestors, but it hides his descendants, and separates his contemporaries from him; it throws him back for ever upon himself alone, and threatens in the end to confine him entirely within the solitude of his own heart.

Despite these caveats, hologistic theory testing is a useful corrective to personally biased nonhologistic studies. Gardner's paper promises to be seminal.

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This article presents something of a paradox. On the one hand, Gardner tries to construct a model explaining why foragers have an "individual-autonomy" syndrome; on the other hand, he presents us with evidence toward the end of the paper that casts doubt on the existence of such a syndrome and its tendency to be characteristic of foragers. First, only 22% of the foragers he examines have such a "syndrome," by his own reckoning. Second, the correlations he presents between elements of social organization that should be related are typically very low; even if they are significant, variation in one aspect explains very little of another. What is the usefulness, then, of postulating such a syndrome? Why wouldn't it be better to "unpack" the elements and ask questions such as the following: Why do some foragers emphasize the individual more than others? Why are some more egalitarian? Why do some share more? Further-

more, why treat foragers separately, as if they were a separate "species" of society? Societies other than foragers (e.g., some Western societies) emphasize the individual in child rearing. Some nonforaging societies are also egalitarian. Many other societies share extensively. Thus, if we want to understand some of the components that Gardner is interested in, it would be appropriate to study them across the complete range of cultural variation.

Although I question the usefulness of postulating a syndrome such as "individual autonomy," if one is going to do so the question of how to measure it has to be taken seriously. First, we need a theoretical definition of the construct. (Gardner provides little or no theoretical justification for grouping traits such as emphasis on the individual, egalitarianism, and flexibility of social organization.) Then, especially with an abstract concept of this kind, we need measures that tap various domains of it (e.g., degree to which the individual acts with others in hunting, in fishing, in gathering; degree to which the individual makes decisions about mate choices; degree to which the individual controls his/her sexual behavior, etc.). Finally, it is only after we gather data on particular indicators of those domains that we can begin, using techniques such as factor analysis, optimal scaling, etc., to validate whether the supposed syndrome is unidimensional or multidimensional. For example, after Whyte (1978) measured the status of women in 50 or so different ways, he discovered using factor analysis that there was little justification for employing the unitary concept "status of women." If we can't measure the concept validly, we can hardly search for correlates of it. (For a more extensive discussion of measurement in cross-cultural research, see Ember et al. 1991.)

A couple of Gardner's statements are incorrect. First, he says that the relationship between migratory bands and/or equality of inheritance is not fully explained in our paper (Ember and Ember 1972). In fact, we discuss and show the separate relationships for migratory bands and multilocality and for equality of inheritance and multilocality, and it is clear that the data used come from available published sources. Second, in discussing our results he seems to equate multilocality with bilaterality. Residence patterns and kinship reckoning are separate domains of social structure which are *not* isomorphic and may be explained differently, as our research suggests (Ember and Ember 1983). Societies with multilocal residence sometimes have unilineal kin groups, and societies with unilocal residence sometimes are bilateral.

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We owe a debt to Gardner for compiling and evaluating alternative theories about individualism, egalitarianism, and organizational flexibility among human foragers.

While his conclusions are interesting and somewhat anticipated, I am bothered by epistemological concerns: the way he seeks to know about foragers and the broader implications of his findings.

Gardner's work shares with many other hologetic cross-cultural studies a minimum of concern for historical context and dynamics, instead deriving generalizations from statistical comparisons of the ethnographic observations of others, made during relatively brief fieldwork in a few locations and presented as static typifying characteristics for entire cultures (Wobst 1978). In this analysis the individual makers and shapers of each culture are invisible, even though they are the actors upon whom selection operates to create evolutionary change. In Gardner's analysis their actions remain neatly sealed within cultural boxes, and change is evaluated through comparison. As he recognizes, such comparisons run the risk of simply projecting the shared biases of ethnographers. Stressing objectivity, methodology, and statistics and using global samples, they often produce an image of scientific rigor and certainty.

By minimizing the input from biological anthropology and archaeology, our source of information for the millions of years that foragers lived in a world of foragers, Gardner is left to compare what are mostly marginalized foragers—operating in a capitalist world fed mostly by domesticated sources of food. And, as he realizes, the cultures being compared are by no means historically unchanging. We should consider why a particular ethnographic present has been selected to typify an entire culture's history. Wilmsen (1989) has recently examined the historical processes by which the San of the African Kalahari gradually become marginalized and homogenized by other Africans and Europeans. At various times and places the San have been farmers, pastoralists, raiders, and specialized hunters in addition to foragers. Restricting them to being foragers obscures much of their rich history and their efforts to resist and cope with pressures from surrounding African and European-derived societies. I suspect that when other African foragers, such as the Hadza, Dorobo, and Mbuti, have received comparable historical attention, they will be found to have equally rich and varied histories.

The major single variable Gardner examines as accounting for egalitarian organization is food storage. It is interesting that the various Plains Indian groups represented in his sample—Gros Ventre, Ute, Kiowa, and Crow—are in different spheres of the Venn diagram (fig. 4). As far as we know, they all shared an important dependence upon bison and other game animals for storable meat and other products. They all acquired horses (becoming more like pastoralists) and firearms in the historic period and were involved to different degrees with Euro-Americans in the fur trade. But making sense out of even this partial and brief comparison requires investigating and understanding each people's migrations, organizational and subsistence changes, environmental circumstances, and involvement with other peoples (cf. Bamforth 1988, Fawcett 1987). Comparison at the culture level will not elucidate their histories.

It is ironic that Gardner seeks to sort out the various theories that have so often stereotyped foragers and yet evaluates their implications through cultural comparison with little concern for change and internal variation. I do not mean to suggest that we return to cultural particularism. Rather, I mean to encourage efforts by Gardner and others to theorize within a framework that stresses reflection on the circumstances that contribute to our asking certain questions (and not others) and arriving at various (and usually temporary) answers. What we do and say as scholars and as scientists affects many others in fundamental ways.

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Gardner rightly notes that within forager studies most theories about the causes of so-called individualism are more than two decades old and that few theorists have engaged in critical dialogue about them or about such widely used concepts as egalitarianism, individualism, and atomism. He aims to link a large number of theories that have until now been seen as distinct, even conflicting. He skillfully shows that these theories are complementary and then uses their complementarity as support for an argument about multiple causality and feedback. My major argument here is not so much with what he says as with what he does not say about the categories employed in the individual-autonomy syndrome.

The paper offers insights into the characterization of forager societies as "individualistic" or "egalitarian" and provides some warnings about essentialist definitions of foragers. Ironically, however, with the exception of one small caveat about the appropriation of "Western" language for the study of foragers, the very concepts of individualism and individual autonomy to which Gardner subjects his critique remain unquestioned. Indeed, his construction of the individual-autonomy syndrome reifies individualism. "Syndrome" is a fitting metaphor for his argument. In medicine, a syndrome is a constellation of symptoms, but in different individuals those symptoms may have different etiologies. In fact, Gardner intends to demonstrate multicausality of the presence of several different cultural features, or symptoms. These features, when found together, form the syndrome, overdetermined yet still a real and objectifiable "thing."

In part because Gardner does not incorporate into his paper the rich literature on the diverse meanings of individualism (e.g., Dumont 1970, Lukes 1973, and Kondo 1990), he risks essentializing individualism and its various components. While he notes that individualism can be defined according to criteria drawn from a variety of cultural systems, it seems to me that the criteria must themselves be culturally defined if they are to have comparative import. Independence, self-reliance, and individual achievement, for instance, are all associated with

the individual-autonomy syndrome, but they appear undefined and taken out of context. Gardner includes "independence" in the same "box" as self-reliance and achievement. But independence is itself made up of different components: the psychological, political, and aesthetic, among others. If one wishes to attribute value to a sweeping term like "independence" as a cross-cultural category, then it has to be more precisely defined and contextualized. Egalitarianism, another important concept for Gardner, remains vague. If we do not know precisely what the units of comparison are, how can we go about comparing? Moreover, if one does not accept the terms of the system, then one cannot accept the "existence" of a syndrome. Gardner asks much of the reader: not only to accept the terms rather uncritically but also to accept that there may still be a way to characterize "foragers" (albeit, as Gardner says, in a limited way) as a distinct type of human society.

Some of the data are questionable because we must accept as valid highly general characterizations about entire societies. In addition, the methodology by which Gardner evaluates the adequacy, applicability, or "codability" (to use Murdock's term) of the *Atlas* is unclear. One additional point—Gardner's definition of foragers as "peoples with 0–5% dependence on either animal husbandry or agriculture"—deserves to be mentioned. First, the accuracy of these figures is suspect because the ethnographers represented in the samples did not all collect quantitative data on foragers' reliance on specific food sources. Second, why 0–5%? Why does an arbitrary measure of "dependence" (whatever that means) determine inclusion in the category "forager"? Furthermore, the sample includes societies, such as the Mbuti of Zaire, for which there is insufficient information on dependence, nutritional or otherwise, but considerable doubt as to whether they have ever lived independent of agriculture (Bailey et al. 1989). Such sampling problems plague most large-scale comparative work (cf. Barnes 1971:66–72), but they are significant enough to warrant questions about the sample used here.

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Gardner is to be commended for this ingenious article on something that "hath oft been known, but ne'er so well expressed." As he shows so effectively in his review of theories formulated by students of hunter-gatherer societies over the past quarter of a century, virtually each one of them has—like the proverbial cat around the hot porridge bowl—talked around the matter of hunter-gatherer individualism or personal autonomy, each from its own theoretical and ethnographic perspective, sensing but leaving inchoate an overall theoretical framework. These various more or less fragmentary theoretical formulations have now finally been integrated

by means of a seminal theoretical construct, the individual-autonomy syndrome.

The construct is shown to organize and explain a wide range of key structural and organizational components of foraging society, such as conflict avoidance through fission, constraints on possession of property, fluid and open group composition, emphasis on the nuclear family, and egalitarianism. Presenting his argument in terms of "multidetermination," Gardner eschews the limitations imposed by theories which assign causal primacy to one of two key variables, subsistence constraints and frontier circumstances. These theories have polarized the field of hunter-gatherer studies for decades, culminating in the current debate between so-called isolationists and so-called interactionists/revisionists. This makes Gardner's article, with its synthesizing and comparative turn, all the more important and timely. Having provided a forum to participants in the great debate just mentioned, *CURRENT ANTHROPOLOGY* is to be commended for featuring an article that has the potential for resolving that debate.

Regarding hunter-gatherers in the frontier situation, it should be noted that the passive, retreating, or emulating stance that Gardner applies so widely to hunter-gatherers in the presence of powerful or hegemonic neighbours was not universally assumed by them. Any such characterization of the forager-neighbour relationship would have to be qualified, for instance, in terms of the nature and degree of domination or oppression inherent in that relationship. Drawing an example from my own area of research, it has been shown by Marks (1972) and Szalay (1981:70–79) for the Cape Khoisan (both Bushmen and Khoikhoi) and by Seiner (1913) and Guenther (1991), respectively, for the Bushmen of north-western Namibia and of the Ghanzi region of Botswana that the aggressive and, at times, genocidal encroachment of white and black settlers on their lands led to the rapid organization of large (up to 1,000 strong) multiband groups under the leadership of powerful and autocratic war chieftains that offered determined and effective resistance to the aggressors. When pushed to the brink, these "harmless" people could indeed be harmful to their oppressors. This fact should be pointed out in order to correct not only the ethnohistorical and ethnographic record on the hunter-gatherers of southern Africa but also the romanticist and patronizing stereotype (as passive and pacifist innocents) that they have at times been cast in by Westerners.

In his discussion of psychological causes and effects of individual autonomy, Gardner looks primarily at such things as emulation, anxiety, and emotional repression as these become manifest in contact with better-organized and oppressive neighbours. The one psychological factor he considers in the context of foraging society per se is the independence training regimen that underlies childhood socialization. At this point one might, following Diamond (1963) and Service (1979:74–75), mention another psychological mechanism for instilling individual autonomy: individuation, the expression and fulfilment of self, by means of the cultural

resources at one's disposal. Given the lack of division of labour and of competitiveness and the relative simplicity of the cultural repertoire of hunter-gatherers with simple societies, individuals can utilize those technological, social, ritual, and expressive elements of that repertoire that they are drawn to and require for their personal fulfilment. Individualism is thus expressed not just formally, by asserting personal autonomy, but also substantively, by manifesting personal creativity. I have written about this matter in the context of Bushman social organization, belief, and witchcraft (Guenther 1986, 1979, 1992).

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My disagreement with Gardner's article is almost total and concerns so many points that I can only mention a few of them here.

1. The problematic. The questions posed by Gardner have to do with what he calls the individual-autonomy syndrome, which includes individualism and equality. He says several times that "the emphasis on individual autonomy" involves (or *can involve*) egalitarianism, and in figure 1 individual decision making and egalitarianism appear in the same box as if they naturally went together. There is, however, no obvious, necessary, or logical connection between individualism and equality. That there may be both egalitarian collectivist societies and inegalitarian individualist ones is a commonplace of Western political thought. Therefore the individual-autonomy syndrome seems to me poorly constructed in that it embraces too much.

2. Definitions. Not only do I contest the implicit linkage of the two notions just mentioned, but I question the sociological relevance of each. Would we, for example, call our own societies egalitarian or inegalitarian? They are egalitarian from the juridical point of view (in that everyone has the same rights) and inegalitarian from the economic point of view (in that not everyone has the same wealth). Here again, the distinction between one level of society (where it is egalitarian) and another (where it is not) is a commonplace not only for the 19th-century socialists but even for a more conservative thinker such as de Tocqueville. It makes no sense at all to speak of "egalitarian society" without specifying one's point of view. This is why I spoke in my first works about the socioeconomic inequalities (only one aspect of social inequalities) associated with differences in wealth, potential control of the means of production, etc. Australian Aboriginal societies, for example, are economically egalitarian, but it is obvious that they have a multitude of inequalities (privileges, differences in prestige and power) between young and old, between women and men, and even between clans (Testart 1989). The terms "egalitarian" and "inegalitarian" can serve to



describe one aspect or another of a society, but they cannot be legitimately used to characterize a society.

There is still more to be said against the sociological notion of individualism, which seems to me rather vague, and I am surprised that Gardner has not felt the need to define it more precisely. Whether individualism can be considered characteristic of hunter-gatherer societies is an entirely different question. These are for the most part societies in which the products of hunting and gathering are shared: is this not a practice that is diametrically opposed to what we ordinarily mean by individualism? Could not one conclude that "individual autonomy" is seriously limited by the feeling of group solidarity? The *obligation* to share is for these societies a principle of law that is the opposite of the 19th-century liberal conception of individualism.

3. The identification of the study set in question and the problem of comparability. In order to work on a set of societies as diverse as those classified as "hunter-gatherers," one must assume that it has a certain unity or at least a certain relevance to the problem posed. I think that the set lacks that unity, and this is the principal conclusion of my research as I see it. This suggests that one must work with smaller subsets or make other cuts. For example, the hunter-gatherers of South Asia or equatorial Africa seem to me to display a number of specific features that are not found elsewhere (Testart 1981:188–203). If some of the "relatively specific theories," such as what Gardner calls the "dominance-escape theory," apparently do not apply to the Australian Aborigines or to the Eskimo, who have remained particularly isolated, why seek to test them on hunter-gatherers as a whole?

4. The treatment of the theories. I do not know whether Gardner is aware of the imprecision with which he handles the theories of others. I would not have recognized myself in the arrow of figure 1, because "the nuclear family," "individual decision making," etc., are things that I have never dealt with. My theory consists only in showing a dual causal relation between two phenomena—essentially demonstrating a major division among hunter-gatherers in terms of economic structure with certain more general social consequences. I do not begin with Steward's work, instead explicitly stating my opposition to his environmentalist ("ecological") interpretation in displaying an economic structure, etc.

5. The statistical method. It may be useful to employ Murdock's codes and to calculate coefficients of correlation, but this can only be an aid to reflection. In particular, it cannot exempt us from the requirement to return to the sources and *comment on them*—that is, examine the conditions under which each society was observed and identify its particularities. It is only in this way that we can assign meaning to a correlation, which never represents more than a very crude approach in comparison with the complexity of the ethnographic facts. Gardner's figures and tables are unusable because they do not tell us which societies go in the direction of the correlation calculated and which in the opposite direc-

tion. Gardner does not provide even himself, much less the rest of us, the means of criticizing his results or going beyond them.

6. The results. Gardner concludes that "preliminary testing of the proposed individual-autonomy-syndrome and multidetermination theories has yielded relatively modest results." I wonder whether it could have been otherwise.

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I would like to begin by commending Gardner for the high degree of analytical rigour that he has brought to a diverse and complex body of hunter-forager theory. I strongly suspect that his point that students of foraging societies often accept colleagues' findings with too little critical thought bears on the fact that few of us dare to plumb these depths—not least, I suspect, because of certain ethnographic boundaries that we have drawn for ourselves.

That Gardner's preliminary testing has led him to only a limited set of theoretical correlations with regard to forager autonomy is heartening to one who sees hunter-gatherers affected by larger, rather than smaller, ecological-cultural systems. I would have been most disappointed if these tests had concluded that any one of these theories was indeed the whole answer.

My own work with Canadian Inuit has led me to conclude that Central Eskimo groups, especially those of northern Baffin Island, with food-sharing, harvesting, and authority patterns developed around extended-family structure and organization, are quintessential practitioners of "interdependent autonomy." Indeed, the Copper Inuit, as detailed in the *Atlas*, may be one the few Inuit-Inupiaq societies capable of fitting so neatly into the "overdetermined" segment of figure 4. I suggest that we need to look further for a more "representative" model of Eskimo society.

Gardner has provided us with much food for thought. At the least, this analysis should help others to sharpen the theoretical focus.

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For an archaeologist such as myself, striving to interpret our hunting-and-gathering past, Gardner's paper provides an excellent review of the different theories current in anthropology about the causes of the individualist and egalitarian nature of forager society. I would like to concentrate on two issues: the notion of egalitarianism and the impact of frontier circumstances on forager societies.

Although Gardner acknowledges the existence of variation among foragers in egalitarian ideology and degree



of social complexity, he perhaps does not go far enough in his discussion of what it means to belong to an egalitarian society. On the one hand, he emphasizes "extreme egalitarianism, including extreme gender egalitarianism," and "a general tendency toward informal arrangements and individually generated, *ad hoc* structures"; on the other hand, he notes "techniques for prestige avoidance and social leveling," remarking that "people in 'loose' societies may, in fact, be highly self-disciplined."

Recent studies are beginning to show that at least some ethnographic forager societies, including those mentioned by Gardner as textbook cases of exceptional egalitarianism (n. 3), are less egalitarian than they may seem to the anthropologist, handicapped by the accepted views of the Noble Savage. Ideology and kinship relationships control forager behaviour, including the procurement and redistribution of resources (e.g., Sahlin 1974). Testart (1988) shows how the social structure of production controls food redistribution and discourages intensification and social stratification. The !Kung San are another classic example (Lee 1979), but even among the San, as well as in other egalitarian societies, food redistribution was asymmetrical (Kent 1989, Speth 1990), generally favouring men and prejudicing the health of women and children (Speth 1990).

To my mind, these observations challenge the notion of the egalitarian society and mark the difference that Speth (1990:148) points out between forager ideology and actual behaviour. This is the key point. Hunter-gatherer ideology acts as a means of *social control* to maintain simple social structure and "egalitarian" society. A set of rules is required to provide disincentives to intensification, accumulation of wealth, and the development of social hierarchies. Despite these rules, differential distribution of resources such as can occur within the ecological constraints of a foraging society favours the survival of some and prejudices that of others.

This means that egalitarian, classless, non-stratified society is not an effortless, natural state; on the contrary, the maintenance of egalitarian societies will incur costs in terms of social control, effort, and energy—costs that are continually under review against the changing pressures and potential benefits of the alternative strategy, the development of social and economic stratification. Such a framework adds a common perspective to the different theories that Gardner describes.

The presence of a frontier, particularly an agricultural frontier, can obviously be very disruptive to the fragile social equilibrium maintained by "egalitarian" forager societies. The different outcomes of the forager-frontier encounter suggested by Gardner's review provide a set of models for application to the past. It is important to note that both a shift to greater social stratification and a devolution towards less controlled social structure can occur. In Europe, the issue of forager-farmer interactions across the Neolithic agricultural frontier has not been adequately explored. This is partly because of the poor resolution of archaeological data as indicators of social organisation and ideology and partly because of the still

prevailing notion that the transition to farming in Europe was a brief encounter between incoming farmers from the Near East and indigenous "egalitarian" foragers ending with the destruction or assimilation of the foragers. Yet some recent studies show that the transition was a complex phenomenon of long duration which involved considerable contact across the prehistoric agricultural frontier (Dennell 1985, Zvelebil 1986, Zvelebil and Dolukhanov 1991).

A cursory survey of the late Mesolithic (hunter-gatherer) societies in Europe reveals both of the developments noted ethnographically: a shift towards greater social and economic complexity, provoked, perhaps, by the social and commercial effect of the agricultural frontier (i.e., Ertebølle and related cultures on the North European Plain and in north-eastern Europe), and a reduction in settlement size, permanence, and complexity, interpreted as a shift towards greater mobility and simpler social organisation (interior areas of the north-eastern Baltic in the 2d and 1st millennium b.c., Central European foragers on the Linear Pottery periphery, etc.). The responses of indigenous foraging societies in Europe to the agricultural frontier may have decided their future evolution, either as adopters of farming themselves or as foraging "survivals" later to be fragmented and absorbed into farming communities. The range of theories presented by Gardner and the contemplation of the egalitarian nature of foragers at large suggest a thought-provoking set of models which can be applied in the specific prehistoric context of the agricultural transition in Europe.

## Reply

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Responses to the initial theory review and to the general purpose and perspective of my paper are encouraging. Although a number of thought-provoking questions have been raised about technical and conceptual aspects of the approach and a few respondents prefer to go in quite different directions, the tone of the interchange on most matters is constructive. I thank everyone for that. My reply concerns (1) method and technique, (2) the individual-autonomy syndrome, (3) individualism and egalitarianism, (4) errors, actual and perceived, and (5) what we might do next.

1. *Method and technique.* Comments on method and technique are diverse. First, although Grinker finds "syndrome" a fitting term for the kind of set it covers (and brings out the reason I used it), Ember notes that the real work of measuring it and establishing the extent of its validity lies ahead. I agree fully with Ember as to the eventual desirability of examining it using factor analysis, as in Whyte's analysis of women's status. When we reach that point, the entirety we now call the syndrome will probably need total reconceptualization,

so we are wise to remember from the outset that we are dealing with a provisional construct.

Dentan and Ember ask if it is appropriate for my sample to be composed only of foragers, their proposal being that we look instead at more diverse peoples that possess the syndrome or major features of it. I have two reactions: (1) Foragers who seek individual autonomy may have some advantages over others in realizing such a system. People with immovable property find their individual decision-making constrained (by the demands of ownership, stewardship, and protection), and even herders move less freely as individuals than the more nomadic foragers. Nonetheless, study of individual autonomy would surely be furthered by use of a sample of the kind they recommend. It would be especially interesting to see how individual autonomy is encouraged and maintained in societies with stored food, other heavy property, and limited mobility. I encourage further inquiry along those lines. (2) The sample employed in the present study is a straightforward function of my dual interest in how anthropologists characterize foragers and how they account for what they see as foragers' characteristics.

Questions are raised about my use of the *Atlas of World Cultures*: Bird-David protests my taking data "uncritically" from it, and Grinker objects to my basing sampling on an "arbitrary" *Atlas* measure of dependence on other than foraging. The *Atlas* was designed with such use in mind; it is a means for avoiding turning a single comparative exercise into a lifelong project. Granted, going back to the original sources ought to allow for the inclusion of more valid particulars, but one would need inestimable time and expertise to reevaluate materials on even a few variables for a world sample. We can learn a considerable amount without doing that; we must simply be aware that many things such as emotional tone are not coded (Dentan), that measurement of variables such as mobility and subsistence needs improving (Cashdan, Bird-David), that standard sampling practices could lead to selection of cultures which are anomalous for their sampling provinces in the variables of interest (Wenzel), and so on. These restrict what we are able to accomplish, but the restrictions are modest compared with those of earlier data bases and sampling methods.

Bird-David's objection to my isolating and comparing the substantive propositional cores of theories is reminiscent of the perennial protest of ethnographers over what happens to ethnographic data in the metalanguage of comparison. A great deal happens. Details and contexts are lost, and terms are transformed. It can be useful, though, to stand back occasionally and view from afar sets of unique particulars, be they cultures or theories (including our own). Such exercises help us discover patterns and frequencies of sharing and diversity. They alert us to some of our biases and let us weigh the soundness of our generalizations. In sum, they facilitate critiquing, one sort of contextualizing, and, if we are lucky, achieving understanding. If we forgo processing data in abstract form because we summarize such operations as

"garbage in, garbage out," we may never reap the benefits of studying our subject matter with one of our discipline's prime instruments for achieving detachment. The metalanguage and the results of such heuristic exercises are arbitrary by definition. So long as we bear that in mind, there is no danger of reification.

Having devoted a semester-long seminar in 1980 to examining both the field techniques which underlay six classic ethnographies and the apparent processes by which their authors arrived at the generalizations we call "ethnographic facts," I can applaud Cronk's remarks on ideology and behavior. We do need to ascertain what kinds of data our ethnographers have relied upon. In addition, Cronk, who asks us to study the reasons for our persisting beliefs about foragers, could benefit from examining the basis for his own beliefs about ethnographies. How did he come by them? What was his data base? He may be relieved to learn that many ethnographers (recent examples are Endicott, Gardner, Lee, Myers, and Nelson) have derived their ideas about foragers' beliefs as much from study of behavior as from records of informants' statements. What they put forward pertinent to foragers' concern with individual autonomy is inferred to a considerable extent from behavioral reality.

In two different paragraphs Ember speaks of my "postulating" the individual-autonomy syndrome, revealing a basic misunderstanding of my method. To postulate is to assume to be true. Yet I have referred to the syndrome, ever since an early (i.e., pretest) draft of the first part of this paper, as a theory in need of testing—the very language I use here, in the abstract, the body of the paper, and the conclusions. Unfortunately, Ember's misunderstanding shapes several of her arguments.

2. *The individual-autonomy syndrome.* Cashdan and Testart hold that more needs to be done on the logic of the relations in figures 1–3. I concur; it is the aspect of the study in most need of further attention. When the figures were drawn, both my approach and my goals limited representation of that logic. My approach was to depict the theories of others. I sought to capture the basics of their theories (or, at times, whole sets of their theories) without filling in missing propositions; if others left relationships unclear, my summaries reflected that. And one of my goals was to keep the figures readable. I tried to avoid great detail even where it might have been provided. Only by such restraint could I expect a reader to grasp the general ideas which underlay the figures. So much hinged on readability that succinctness was accorded greater value than completeness; I fear it was overvalued. A way around the problem might be two-step figures, version 1 being detailed and version 2 being simple enough to bring out the overall configuration.

My respondents require more of an answer than this. They hold that the relationship between individualism and egalitarianism can differ sharply from that which I find ethnographers attributing to foragers. In complex societies, they argue, capitalism leads to individual autonomy with inequality and communism leads to the opposite. From the viewpoint of the syndrome these are

mixed cases. Examples of them certainly should be examined. First, though, we might look at foragers having these same combinations of characteristics (11 are listed in fig. 4), and we should review Orans's (1965:129) position that, for members of simpler systems encysted by more powerful societies, economic emulation will be individual, uneven, and "divisive." I have tried to make it clear that truncated versions of the individual-autonomy syndrome can be attributed to some foragers and that key features of the syndrome are also found independently of it. As for the achievability of individual autonomy in capitalist societies, we must not forget the authorities who regulate behavior in the name of law, order, and protection of property. Although an ideology of "individual autonomy" may be used by the powerful to rationalize a capitalist system, the freedom which less powerful people actually enjoy differs greatly from that which the more anarchic foragers extend to all members of society. This is one comparison in which it is essential that ideology not be confused with behavioral reality. There is food for thought as well in Tillich's (1953) thesis that existentialism, Marxism, and depth psychology are futile protests against dehumanization in technical society: they falsely promise liberation and can give individuals a deceptive feeling of being able to make decisions.

Cronk's positive response to my conclusion that few foragers really can be characterized by the individual-autonomy syndrome is puzzling. It is inconsistent of him to accept even the details of this finding, and build upon it, while questioning the underlying data and the techniques by which it was reached.

3. *Individualism and egalitarianism.* Dentan, Grinker, and Testart regret my lack of attention to the Western concept of "individualism." Ideally, their own reviews will suffice. While I share their interests, I am certain that the editors would cut from the paper an interpolated essay of any depth on this on the grounds that it was extraneous to my main arguments.

I was stimulated by Testart's remarks on the obligation to share the products of hunting and gathering and the possibility that individual autonomy is "seriously limited by the feeling of group solidarity." To be sure, "group solidarity" describes inadequately the apparent feelings of band members at the actual moment when food is distributed among !Kung (Draper 1978; Lee 1969, 1979; Tanaka 1980), Mbuti (Turnbull 1965), and so on. Directions, accusations, and noisy claims dominate those scenes. But the outbursts (which Lee [1969] sees, I believe correctly, as intentional obstacles to the development of pride and immodesty in providers) might, in the long run, be among the prerequisites for achieving a degree of solidarity. The extent to which solidarity limits individual autonomy is a worthy subject for future inquiry. As I have already noted, there are institutions which limit individual autonomy in Western capitalist society, and experience in both kinds of system suggests to me that people here have to cope with considerably greater limitations.

Testart and Zvelebil challenge the general labeling of

societies as "egalitarian." Their points are well taken, especially as regards the need for attention to dimensions of inequality. Zvelebil is right, too, that I do not go far enough in discussing ways in which "textbook cases" sometimes fall short of full-fledged egalitarianism.

There is another side to this which bears mention. I find that we customarily fail to acknowledge just how extreme and general egalitarianism can be. After 19 months with Paliyans, I flinch when reading that certain foraging people "are egalitarian except, of course, in regard to age and gender." Paliyans do not achieve *perfect* equality in these two dimensions, particularly in contact settings, but they come closer to doing so than most social philosophers dare dream of. A six-year-old Paliyan girl is a person, with a person's rights that even nonrelatives will help protect. Zvelebil is correct that "egalitarian, classless, non-stratified society is not an effortless, natural state." His comments are astute, and I am glad that archaeologists such as Speth and he are giving attention to the subject. Earlier work on social control (K. L. Endicott 1986; K. M. Endicott 1979, 1986; Gardner 1966b; Lee 1969, 1979; Turnbull 1965; Woodburn 1968, 1982) reminds us, though, that actual behavior may not lag far behind ideology. Foragers generally have whole sets of practical control mechanisms, and, with the expenditure of some effort, they can be made highly effective.

Three others (Cronk, Ember, and Guenther) make perceptive comments on individualism or egalitarianism. I acknowledge their contributions but find myself unable to reply to each of them individually.

4. *Errors, actual and perceived.* Four matters need attention. Reexamining Ember and Ember's (1972:386) footnote on whether migration and equality of inheritance, together, predict multilocality, I find it possible both to infer their sample size and to ascertain their reason for dealing with the two variables on an "and/or" basis. I did not read it before with sufficient care, and I apologize for my mischaracterization.

Barnard, without claiming to correct an error, refers to Woodburn as a storage theorist. While figure 2 acknowledges Woodburn's concerns with storage (arrow m), the text of my paper discusses only his reexaminations and critiques of frontier theories. Even a double classification may understate his contribution. Indeed, Woodburn's work defies easy summarization. He eschews simple causal explanation, and his theoretical concerns are broad-ranging (as attested by fig. 2). Probably many of us view him as working in our respective areas. Storage for Woodburn, though not an independent variable, is intimately linked with social organization, and he believes that it has to be looked at in environmental, historical, and cultural evolutionary contexts. Barnard's stance is justified, and I am glad to draw attention to it.

Testart disagrees with two of my summaries of his theory. His difficulty in recognizing his own views in figure 1 stems, apparently, from overlooking the word "or" in the large box near the bottom. The figure sum-

marizes many people's ideas in a few words, and it is a mistake to attribute the whole set of ideas to each individual. The other disagreement concerns my saying, "Beginning with Steward's (1968:328) seasonal-scarcity explanation of food storage in higher latitudes, Testart (1982:528) hypothesizes. . . ." The references were given deliberately because this is not Steward's best-known position. Steward contrasts (a) the constant availability of vegetable resources in low latitudes and the difficulty of storing foods in heat and humidity with (b) the seasonal need for storage and the availability of techniques for it in higher latitudes. Except that Testart notes a lack of large-scale storage among higher-latitude land hunters, he maps out a distinction similar to Steward's, between (a) desert and tropical hunter-gatherers, for whom he suggests that bounty of food resources and periodic scarcity or seasonality of foods, respectively, are insufficient "to induce storage," and (b) high- and medium-latitude hunter-gatherers, who, he holds, need to store food because of its seasonality and who possess adequate techniques for doing so. What he does next is different, but Testart *begins* with a set of variables and a distributional explanation enough like Steward's to warrant notice.

Ember is right about the lack of clarity of my statement that Ember and Ember "deal with a relationship (i.e., egalitarianism → bilaterality) which has no counterpart in figure 3." It is overly succinct, and it is phrased wholly in terms of symbols and components in my figures 2 and 3. First, "relationship" refers to the arrow. Second, there was no such arrow in figure 3 (or in figure 2). Third, I recognize three *Atlas* variables, including residence, as being pertinent to social structure; although Ember and Ember put matters differently, their finding that equality of inheritance helps predict multilocality pertains directly to two of my components, egalitarianism and bilaterality.

5. *What we might do next.* Several suggestions for further work have been acknowledged already. In addition, there are these:

Cashdan has referred to recent documentation among San (Draper and Cashdan 1988) of changing participation of children in sex-typed work and changing modes of child rearing. It is clear that additional longitudinal studies and comparative studies along the same lines would contribute much to our understanding of the autonomy-seeking child. The Draper and Cashdan paper is far more than a document of change, however. Its final pages are replete with specific proposals on how to extend such research productively.

Guenther's reminder about individuation is well put. MacKenzie Drainage Dene examples come to mind immediately, giving added salience to the idea, and I can see that it has the potential to counter the preoccupation many of us have with autonomy. Including individuation in the syndrome would also oblige us to rethink some of the processes supposedly linking syndrome features. Individuation is, of course, going to be more apparent in some foraging societies than others. As in many peasant societies, countervailing pressures can be strong

against doing anything which sets one apart from one's neighbors.

There is a certain appeal to Barnard's idea that my "individual-autonomy theory represents, for the *forager* rather than the anthropologist, one perception of how society should work." There is also a troubling side to it. Although foragers did, in effect, give me the theory, they demonstrated the principles by means of their behavior long before I ever heard them expressed in words. The implications of calling this one of their perceptions require further thought.

Following one of my own closing recommendations—that we employ Foulks's model and return to a case-by-case study of foragers "with their diverse natural settings and contact histories . . . for ascertaining with any certainty which factors are most important in shaping each case"—would allow us to approximate an approach which Fawcett and Testart recommend. Let it be clear, though, that this is not a call to retreat from comparison but a suggestion of something akin to what Dentan requests, a historical study to complement the present one. We would find ourselves examining subsets of foragers (Testart) and different outcomes of frontier contacts (Guenther, Zvelebil). In this connection, I thank Guenther for his references to foragers that have offered resistance to encroachment. Another such case is the Chenchu of peninsular India. They acquired muzzle-loaders at a time when encroachment intensified, then developed a reputation for using them (Aiyappan 1948:152, 154; Thurston and Rangachari 1909:27–29; von Fürer-Haimendorf 1943:33, 289).

Cronk and Fawcett stress the importance of our reflecting on why we typify our subjects and why we ask questions as we do. Considering the insights resulting from such reflection in the past, the idealist in me shares their view. Nevertheless, practical experience teaches me that achieving any degree of validity (and usefulness) in our answers to questions such as theirs would necessitate some involved inquiry and a separate full-length paper. Whoever undertook the project would have to do something on the order of identifying and examining anthropological innovators in their intellectual environments, semantically analyzing sets of anthropological terms in cultural context, or, at the very least, studying historically and regionally defined traditions. Anthropologists have long since outgrown accepting global, timeless characterizations of their beliefs, and they would be the readers.

The commentators make no additions to the roster of theories, and, with the exception of Testart, they seek no changes in the capsule descriptions of the theories. These responses and the many positive reactions to the perspective suggest that there is something here to build with. That is gratifying. Questions were expected on the methods, the definition of the syndrome, and the establishment of directions for future inquiry. A number of the questions received reflect our theoretical and methodological differences, but that does not diminish their importance. Although I have attempted to offer a brief reply to each, it is clear that many of them are questions

or challenges for all of us. I hope that we will see others answering them in due course.

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